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Vol. VIII, No. 29

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THE ADVANCEMENT OF SCIENCE

VOL. VIII, No. 29 JUNE, 1951

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The Edinburgh Meeting

The Preliminary Programme of the Annual Meeting at Edinburgh, August 8–15, has been published and widely circulated. At the time of writing the rate of registration of members who intend to be present at the meeting far exceeds the rate of registration at the corresponding time in previous years and there is every indication that the attendance will be one of the largest in the history of the Association.

The Association has already met five times in the Capital of Scotland, in 1834, 1850, 1871, 1892 and 1921. After this meeting in 1951 Edinburgh will join Birmingham in the distinction of having received the Association six times. This is a fine record of hospitality and generosity. Many members who attended the meeting in 1921 (when the Association was firmly re-established after the First World War) and a few who were present at the meeting in 1892, are looking forward to this special meeting under Royal Presidency in the year of the Festival of Britain.

Edinburgh extends to fifty square miles between the Pentland Hills and the Firth of Forth. The old Town on the ridge of the Royal Mile, between the Castle and Holyrood, now stands in the centre of a modern city that has spread over a series of seven ridges and the intervening valleys. The city is the seat of the supreme courts, of the General Assembly of the Church of Scotland, and of many national institutions for the advancement of science and the arts, including the Royal Society of Edinburgh (1783) and the Royal Scottish Academy. Surrounded by a galaxy of famous schools and colleges stands the University, founded as the Town's College in 1582.

To the contrast between the picturesqueness of the Old Town and the Georgian elegance of the New Town—with its associations with Hume, Adam Smith, Boswell, Scott and Raeburn—is added the contrast between the Edinburgh of tradition and the less widely known industrial city of half a million inhabitants. The significance of the city as a focus of not only regional but national interests is reflected in the fact that it is the home of one of the two national dailies.

The many cultural institutions include numerous libraries, amongst which may be mentioned the National Library of Scotland, the Signet Library, and an excellent Central Public Library. In the heart of the city are the National Gallery of Scotland, the National Portrait Gallery, the National Museum of Antiquities and the Royal Scottish Museum. Within easy reach of the heart of the city are the Royal Botanic Garden, noted for its beauty as well as its scientific interest, and the Scottish Zoological Park which displays a collection of great interest on an outstanding site. Edinburgh is now also the home of a world-famed Festival of Music and Drama the fifth of which opens on August 19.

An extensive programme of excursions and visits to points of interest within the city has been arranged and it will also be possible for members to make tours which will visit Oban and the Western Highlands, Glencoe, Pitlochry, the Trossachs and the Border country.

A scientific survey of South-East Scotland has been specially written and will be presented to all members who attend the meeting. This Survey is one of a series prepared for the Association's Meetings and, recognizing their unique interest and their lasting value as reference books, the Council is grateful for the local generosity of money and skill which makes their production possible.

The scientific programme of the meeting was drafted in complete detail by the end of May and will be posted in mid-July to members who have registered for the meeting. It contains the usual wide range of subjects sponsored by the Association's thirteen Sections and other departments. As is appropriate in a year when the whole

country is displaying the British contribution to civilisation in the arts, in science and in industrial design, the programme includes many items which will deal with the progress of British Science during the century since the Great Exhibition. For example, H.R.H. The Duke of Edinburgh will deal, in his Presidential Address, with *The British Contribution to Science and Technology during the past Hundred Years* and seven of the Sectional Presidential Addresses will review progress in particular fields.

These and other reviews will not, however, present merely a chronicle of past achievements, for one of the objects of the present stocktaking is to inspire further progress in the future. One contemporary journal recently made the following suggestion : ' There could be no better celebration of Festival year than to make it the opening of a new era for British Science, an era in which this country, not regretting past glories but using the gifts and resources she has, will again take leading rank in the search for knowledge and the use of it to meet human needs.' British men of science have pioneered and continue to lead in pure science but in the applications of useful knowledge the lead has passed to others. This is a problem of post-war reconstruction which has been considered at several meetings of the British Association and will again be considered this year in the unique forum which the great meetings of the Association provide.

On the occasion of the meeting in Edinburgh the President will receive the honorary degree of Doctor of Laws from the University, and on May 3 His Royal Highness was admitted to the Fellowship of the Royal Society. Gratified by these marks of esteem for their President the Members of the Assocation share the honour which they confer.

Meetings of Other Associations for the Advancement of Science

Early in April Sir John Russell (President 1948) returned from a journey in which he travelled over 25,000 miles in Africa, India and Pakistan on a mission sponsored by the British Council. He was present as the Association's representative at the annual meeting of the Indian Science Congress Association at Bangalore in January, at which there were many interesting papers, lectures and discussions. At the opening meeting, at which Pandit Nehru was present, there was an enormous concourse, estimated at 8,000 people in a huge temporary structure of matting on poles, but the loudspeaker arrangements were so excellent that everybody present could hear.

The annual meeting of the French Association for the Advancement of Science was held at Tunis in May when the Secretary represented the British Association. The attendance at this overseas meeting was smaller than is usual at meetings in metropolitan France, but it was judged to be a most successful expedition by those who were fortunate to be present. Under the presidency of the distinguished mycologist M. Roger Heim, the chief emphasis in the scientific programme was on the biological side. A delightful feature of the meeting was the arrangement of long excursions into the interior which gave visitors an excellent impression of a country making great progress, with the aid of science, in the struggle against the desert. In 1952 the French Association will meet in Caen where members of the British Association will be assured of a warm welcome.

As is usual, the Council have invited other Associations to be represented at the meeting of the British Association. It is expected that these will be entertained as guests at Edinburgh, ten representatives from the Associations in Australia and New Zealand, India, Pakistan, South Africa, France and the United States.

The work of Charles Lapworth: Corrigendum

In the report of a symposium on the work of Charles Lapworth, which was published in the March issue of *The Advancement of Science*, two lines were unfortunately deleted after the word ' Wrekin' in line 28 on page 434. This has the effect, in what follows, of misrepresenting some of Lapworth's research work in Shropshire. The two lines were ' Callaway had already investigated the Wrekin area and had given the name Uriconian to the igneous rocks of the Wrekin.'

THE GREEN PLANT[1]

by

PROF. WALTER STILES, F.R.S.

WHEN the prophet Isaiah more than 2,500 years ago said ' All flesh is grass ' he was making a statement which was *literally* truer than he probably intended. For the green plant not only absorbs the raw materials of air and soil and builds them into its own body, and so manufactures the substances necessary for its own growth, but its body provides the food, either immediately or ultimately, for practically all non-green plants and all animals. With the exception of a very few lowly organisms such as some of the sulphur bacteria, all life is dependent on the activity of the green plant. Because it is green, that is because it contains the green pigments known collectively as chlorophyll, it is able to synthesise sugar from the carbon dioxide of the air and the water absorbed from the soil. To do this energy is required, the energy being provided under natural conditions from sunlight.

Some of the energy stored in the sugar so produced is utilised, by means of the breaking down of the sugar molecule in the process called respiration, to produce all the multifarious other substances which make up the plant body, and for that matter the animal body as well. This production of sugar from carbon dioxide and water by the green plant, the process known as photosynthesis or carbon assimilation, has justly been considered the most important of all manufacturing processes, for life on this planet is absolutely dependent on it. Not only so, but its activity ages ago resulted in the production of plants and animals, the remains of which form coal and so-called mineral oil. The energy stored in these through photosynthesis ages ago provided the means for the great industrial developments of the nineteenth and twentieth centuries.

It is interesting to consider for a moment the magnitude of this process, and here I might quote some figures given recently by Professor Walter Loomis of Iowa. He gave the world production of steel as about 100 million tons a year. The quantity of carbon dioxide fixed by land plants over the surface of the earth is estimated at about 68,000 million tons a year. But the oceans also contain large quantities of plants, for the most part microscopic. The amount of photosynthesis taking place in the seas of the world is problematical, and estimates vary from less than that of land plants to several times as much. Rabinowitch estimated the photosynthesis by plants on the ocean to be eight times that of the vegetation of the land. If, however, we take a moderate view and assume the photosynthesis of the ocean flora per unit area is only twice that of land plants, we reach a figure approaching 300,000 million tons of sugar manufactured every year by the activity of green plants, a solemn thought for us who still have to endure sugar rationing. Without question photosynthesis is the world's greatest manufacturing process.

Although the part played by the green plant in providing the food of man and other animals must have been tacitly realised from early times, it is only comparatively recently that the place of the green plant in the cosmic scheme, in making the energy of solar radiation available for human effort, has been fully understood. It is scarcely possible to say when this fundamental position of the green plant was realised. Certainly this aspect of its activities was not appreciated by the pioneer scientific workers of the eighteenth and early nineteenth centuries, Priestley, Ingenhousz, Senebier, de Saussure. These men who discovered during the latter part of the eighteenth century the external sign of photosynthesis, that green plants, or the green parts of plants, give out oxygen and absorb carbon dioxide in the light, were rather impressed with the hygienic

[1] Semi-popular lecture to the Botany Section at Birmingham on September 4, 1950.

attributes of the process than with anything else. Note the title of Ingenhousz's work of 1779 : ' Experiments upon Vegetables, discovering their great power of purifying the common air in sunshine and of injuring it in the shade and at night ; to which is joined a new method of examining the accurate degree of salubrity of the atmosphere ' and the statement in the preface of that work ' The discovery of Dr. Priestley that plants thrive better in foul air than in common and in dephlogisticated air ' (i.e. oxygen) ' and that plants have a power of correcting bad air, has thrown a new and important light upon the arrangements of this world. It shows that the air, spoiled and rendered noxious to animals by their breathing in it, serves to plants as a kind of nourishment.'

At the beginning of the nineteenth century de Saussure showed by quantitative measurement that green plants in the light absorbed carbon dioxide from the air and at the same time gave out oxygen, but he never realised that the whole of the carbon in the body of the plant came from the air. That fact was not finally accepted until about half a century later when Boussingault, Sachs and Knop by using the methods of sand culture and water culture, showed that plants could be grown from the seed to maturity if the roots were supplied with nothing more than a weak solution of mineral salts containing no carbon, thus proving that all the carbon in the plant must have come from the carbon dioxide of the air. What happened to the carbon dioxide after it had been absorbed was an open question until Sachs in 1862 confirmed what had been suspected without proof many years earlier, that starch was the first visible product of photosynthesis. By means of the well-known iodine test he was able to show that starch developed in green leaves after they were exposed to light, and he saw in this starch the result of the reactions involving the carbon dioxide absorbed by the leaves and the water absorbed from the soil. Subsequently it was shown, particularly by Meyer, that some plants did not form starch, and that in these sugars accumulated. There is still controversy as to which sugar is first formed, for leaves generally contain a mixture of these ; whether it is a hexose (glucose or fructose) or a mixture of hexoses, or sucrose (cane sugar) ; but in any event the sugar is a much less complex substance than starch, and so it is generally supposed that where starch is the first *visible* product of photosynthesis it is preceded by sugar ; and since hexoses are simpler substances than the disaccharide sucrose it is often presumed that hexose sugar such as glucose precedes the formation of sucrose or starch. Although from time to time arguments have been advanced in favour of some photosynthetic product other than sugar or starch most plant physiologists would still, I think, accept the photosynthetic process as represented by the well-known equation

$$6\,CO_2 + 6\,H_2O = C_6H_{12}O_6 + 6\,O_2 + \text{light energy}$$

The amount of energy utilised will be immediately appreciated from the fact that about 1,700 kilogram calories are required to make a pound of sugar ; this equals the energy required to raise the temperature of about 4 gal. of water from 0° to 100°. The equation is supported by measurements of the gases absorbed and evolved, the ratio $\frac{O_2 \text{ evolved}}{CO_2 \text{ absorbed}}$ being nearly always unity, and by analyses of photosynthesising material. This may then be regarded as the overall equation of the world's most important manufacturing process.

Before, however, passing on to a consideration of the mechanism of photosynthesis it may be interesting to consider briefly some consequences of the gaseous exchange between the green plant and the atmosphere that was the main consideration of Ingenhousz and his contemporaries.

The atmosphere contains about 0·03 per cent. carbon dioxide and about 21 per cent. oxygen. By photosynthesis green plants tend to reduce this quantity of carbon dixoide and increase the amount of oxygen. The respiration of both plants and animals has the reverse effect, while by the decay of dead organisms the carbon in them is ultimately returned to the atmosphere as carbon dioxide. It has been pointed out by Gaffron that as the earth cooled, when the temperature fell below 100° C. the atmosphere contained

little or no oxygen but plenty of carbon dioxide. If this is true, the oxygen now present in the atmosphere has been provided by photosynthesis. It has been calculated that the present rate of photosynthesis by green plants is sufficient to produce this amount of oxygen in a few thousand years. It seems, however, that the first living things could not have carried on photosynthesis in the way that green plants do, because not only is the chlorophyll system a very complicated one, but it would seem to depend on the products of photosynthesis for its formation.

There is surely no need to emphasise the importance both from a purely scientific point of view and from an economic aspect, of discovering how this synthesis of sugar from carbon dioxide and water is brought about, and for close on 90 years investigators have sought the solution of this problem, and more than one theory which has later proved untenable has been put forward.

Microscopic examination of the organs of the plant in which photosynthesis occurs has not shed much light on the mechanism of the process. The laboratory or factory is the green cell. In lowly unicellular algae such as *Chlorella* and *Protococcus* this is the whole plant, but usually the photosynthesising organ, such as the green leaf, comprises a large number of cells. But whatever the kind of plant the green pigment is always localised in bodies, the plastids, composed of fatty and protein substances. Various views have been held of the way in which the green pigment is associated with the ground material of the plastid, but the significance of the association is not understood.

It is clear that for a theory of photosynthesis to be worthy of consideration certain facts must be credibly explained. There are, for example, the part played by light, the role of the green pigment chlorophyll, the mechanism of the reduction of carbon dioxide, for the production of sugar from carbon dioxide and water clearly involves this. Further, it would seem fairly obvious that the building up of hexose, a substance with six carbon atoms in the molecule, from carbon dioxide, a substance with only one carbon atom in the molecule, is extremely unlikely to take place in one reaction, but is almost certainly brought about through a number of steps. All these conditions were fulfilled in the formaldehyde theory which for about half a century dominated the minds of most plant physiologists concerned with the problem of the photosynthetic mechanism. This theory was launched by the eminent organic chemist Baeyer in 1863. He supposed that carbon dioxide was dissociated into carbon monoxide and oxygen by the action of light, and that the carbon monoxide was then bound to chlorophyll in the same way as that gas is bound to haemoglobin ; the chemical relationship of chlorophyll and haemoglobin is well known. The carbon monoxide was then reduced by hydrogen to formaldehyde ; the origin of this hydrogen was not stated by Baeyer but presumably it was supposed to come from water, and later writers have suggested that this was also brought about by light. The formaldehyde would then, under the influence of substances in the cell, be condensed to sugar, as it can be through the action of alkalies outside the plant. The Baeyer theory can therefore be represented thus :

$$CO_2 \xrightarrow{\text{light}} CO + O$$

$$CO + \text{chlorophyll} \longrightarrow CO.\ \text{chlorophyll}$$

$$H_2O \longrightarrow 2H + O$$

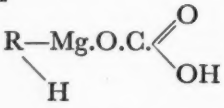
$$CO.\ \text{chlorophyll} + 2H \longrightarrow H.CHO + \text{chlorophyll}$$

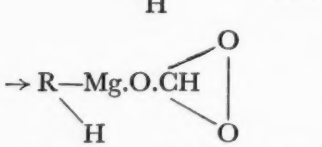
$$6\,(HCHO) \longrightarrow C_6H_{12}O_6$$

We have here parts provided for light and chlorophyll, the ratio of oxygen evolved to carbon dioxide absorbed is unity, as experimentally observed, and the whole process of sugar production involves a number of different reactions. However, in spite of the very great deal of work which was done to establish the existence of these various supposed stages in the photosynthetic process, actually no unquestionable evidence was ever obtained that any one of these actions took place in the green plant. And further, although numerous attempts were made to show that formaldehyde was present in plants following illumination, or could be produced outside the plant in systems containing carbon dioxide and chlorophyll, none of these supposed successful attempts withstood sound criticism. And when by

1918 the theory, after a successful run of 55 years, was showing just a little sign of flagging, fresh vitality was given to it by Willstätter and Stoll, whose views had to be treated with much respect after their outstanding performance in the isolation, preparation and investigation of the properties of the pigments to which green plants owe their colour. The version of the formaldehyde hypothesis of Willstätter and Stoll supposed that carbon dioxide, having combined with water to form carbonic acid, then formed an addition compound with chlorophyll, thus producing a substance of bicarbonate type. This compound, absorbing light energy, became isomerised into one of a peroxide constitution, and this, being acted upon by an enzyme of peroxidase type, broke down into chlorophyll, formaldehyde and oxygen. Willstätter and Stoll had already shown that green plants contained two green pigments (chlorophylls *a* and *b*) and that these contained magnesium, so we can now represent either chlorophyll as R=Mg. We can then summarise the Willstätter and Stoll theory thus :

$$R{=}Mg + H_2O + CO_2 \longrightarrow$$





$$\rightarrow R{=}Mg + H.CHO + O_2$$

The formaldehyde was then supposed to condense to hexose sugar as Baeyer had hypothesised more than half a century earlier. This theory, like Baeyer's before it, found a place for chlorophyll and for light, and it also fulfilled the necessary conditions regarding the relative quantities of carbon dioxide absorbed and oxygen evolved. For the rest it was based on very little more in the way of fact than the theories that had preceded it. In fact, Willstätter and Stoll, although they had made a contribution of the first magnitude towards our knowledge of the pigments concerned in photosynthesis, contributed very little towards an analysis of the complex of actions which it seems clear are involved in the manufacture of sugar by the green plant.

In the meantime F. F. Blackman and his associates had made a contribution of the first importance to the analysis of the photosynthetic process. They had shown that under conditions of high light intensity and high carbon dioxide concentration, the rate of photosynthesis by green leaves is about doubled for a rise of 10° C., whereas in low light intensities the rate of photosynthesis is proportional to the light intensity but is not affected by rise of temperature. That is, with the former set of conditions the rate of photosynthesis is affected by temperature in the same way as a purely chemical (or dark) reaction, while in low light intensities it is influenced by light and temperature in the same way as a photochemical reaction. This work thus suggested that there must be involved in photosynthesis a purely chemical reaction independent of light, as well as the photochemical reaction indicated by the dependence of the whole process on illumination. In low light intensity the low value of this factor limits the rate at which the photochemical process can proceed and so this controls the rate of the whole process ; in high light intensity this factor being in excess, further increase has no effect, and the factors involved in the dark reaction being in relatively short supply it is the dark reaction, which is influenced by temperature, which controls the rate of the whole process. The purely chemical reaction was called the Blackman reaction by Warburg in honour of its discoverer. It will be observed that both the Baeyer and Willstätter and Stoll versions of the formaldehyde theory involve both photochemical and dark chemical (including enzyme) actions.

Now the method of Baeyer and of half a dozen or more other theorists up to Willstätter and Stoll, was to formulate a scheme of reactions which were in agreement with certain facts. Unfortunately they were not supported by others. Such theories can, however, be of value if they lead to further experimental work directed to correcting the theories where they are found to be incompatible with observed facts. It must, however, be admitted that the formaldehyde theory did not inspire work in this way, and I think there is little

doubt that it proved to be a very considerable hindrance, rather than a help, in elucidating the problem of the photosynthetic mechanism. And it has been, in classic phrase, an unconscionable time a-dying. Even now it is liable to crop up in the answers of candidates in botanical examinations at all levels.

Another way of tackling the problem is to try to analyse and, if possible, isolate various parts of the whole process. It is, to my mind, the great achievement of F. F. Blackman and his school that they effected the first step in this direction in demonstrating the existence in the whole photosynthetic process of two diverse actions, one purely chemical, the other photochemical. Another step was taken by R. Hill in work with isolated chloroplasts. The green pigment essential for photosynthesis is, as we have seen and as every botanist knows, not diffused uniformly throughout the cell, but is contained in small granules, the chloroplasts. Attempts had been made in the past to discover whether these granules, when isolated from the cell, could photosynthesise, but it remained in doubt whether any of the attributes of photosynthesis could be recognised as associated with them, although there *was* evidence, not very clear certainly, that on exposure to light a faint evolution of oxygen from them did actually take place.

Now in 1939 Hill published the results of experiments in which he exposed isolated chloroplasts from chickweed and white deadnettle to light when they were suspended in a solution of haemoglobin. I may recall that haemoglobin, the pigment of the red blood corpuscles, absorbs oxygen with the formation of the brighter red pigment oxyhaemoglobin. The spectrum of oxyhaemoglobin is different from that of haemoglobin, so if the isolated chloroplasts give out oxygen this is absorbed by the haemoglobin to form oxyhaemoglobin and the change can be followed by observing the spectrum of the solution, which can be done quantitatively by means of the spectrophotometer. In this way Hill showed that if leaf extracts were present, oxygen was evolved by the chloroplasts, and, what was more significant, that the leaf extract could be replaced by a ferric salt which was reduced to the corresponding ferrous salt with evolution of oxygen. Thus ferric oxalate was reduced to ferrous oxalate and oxygen evolved. That is, the chloroplasts contain a mechanism by means of which light energy is absorbed and thereby a substance, the ferric salt in Hill's experiments, is reduced and oxygen contained in it released

$$Fe_2O_3 \longrightarrow 2\,Fe\,O + O$$

Other workers have since shown that isolated chloroplasts and chloroplasts broken up into pieces, called grana, can bring about the reduction in the presence of light of a number of other substances, such as quinones.

Now these results of Hill suggest that the actual mechanism for the absorption of energy is located in the chloroplast and that connected with this absorption of energy some substance is reduced in the same way as the ferric salt or quinone in the experiments made *in vitro*. Evidence for this is found in the fact that the evolution of oxygen by isolated chloroplasts is inhibited by the narcotic urethane, and this substance also inhibits photosynthesis. On the other hand cyanides have no effect on the evolution of oxygen by the isolated chloroplasts nor have they on the photochemical stage of photosynthesis. Last year Arnon and Whateley reported that three other substances, hydroxylamine, sodium azide and phenyl methane, all of which inhibit photosynthesis, also inhibit the evolution of oxygen by isolated chloroplasts. There is thus quite a good case for supposing that the mechanism present in the chloroplasts which leads to the reduction of a number of inorganic and organic compounds with evolution of oxygen, is the actual mechanism concerned with the evolution of oxygen in photosynthesis. What substance is reduced in this way during the course of the photosynthetic process we do not know. It might be a substance produced from the carbon dioxide absorbed by the green plant, but it is important to note that the evolution of oxygen under the conditions of Hill's experiments is not accompanied by any absorption of carbon dioxide. Hill thus apparently succeeded in separating the process or processes following on the absorption of carbon dioxide from

the process concerned in the evolution of oxygen ; moreover, the evolution of oxygen appeared to be connected with the reduction process brought about by light energy. The first reactions in which carbon dioxide is involved thus do not appear to be concerned with the absorption of light energy. Thus Baeyer appears to have been wrong in hypothesising a splitting of CO_2 to $CO + O$ by the action of light, and those writers who, in the earlier years of this century, liked to refer to the ' photolysis of carbon dioxide ' as a preferable term to ' photosynthesis ' were also wrong.

A new attack on the problem of photosynthesis became possible in the late 1930's when radioactive isotopes became available for physiological investigations. Such researches started in California, and most, though not all, the work with these isotopes has been done in California up to now. The radioactive isotope of an element, carbon for instance, behaves chemically like the non-radioactive type element, but if it enters the plant it can be distinguished from the element that was there already by its radioactivity, which can be recognised and measured by the instrument called the Geiger counter. For this reason the movement of the element and what happens to it can be followed, and so such radioactive substances are called tracers. They are, as it were, labelled, and so radioactive carbon supplied to a plant is often called labelled carbon. The leader in the early work on photosynthesis with such radioactive tracers was Ruben who with a team of associates, carried out work in which green plants were supplied with carbon dioxide containing radioactive carbon or radioactive oxygen.

A very important conclusion was drawn by these workers from experiments in which the unicellular green alga *Chlorella* was supplied with carbon dioxide and water both containing radioactive oxygen. The carbon dioxide was actually supplied as a bicarbonate, which in solution produces a certain proportion of carbon dioxide, and the alga was exposed to light in a solution of bicarbonate in water. The percentage of bicarbonate containing radioactive oxygen varied in their different experiments, and so did the percentage of the water containing radioactive oxygen. The oxygen given out by the photosynthesis of the alga was collected, and the percentage of it which was radioactive was measured. Now in every one of their experiments they found that the proportion of the oxygen which was radioactive was the same as the proportion of radioactive oxygen in the water and was completely independent of that in the bicarbonate or carbon dioxide. Now this was a most important finding for it shows that the oxygen which is given off by the photosynthesis of the green plant comes from the water and not from the carbon dioxide, and this really gives a death blow to the Willstätter and Stoll theory or any other formaldehyde theory of photosynthesis.

For consider the equation

$$CO_2 + H_2O = CH_2O + O_2$$

which summarises such theories. It is clear from this that not more than half the oxygen given off can come from water; indeed all of it might come from the carbon dioxide and none from the water. Since in fact it all comes from the water the formaldehyde theory cannot be true. This work of Ruben's has since been repeated by other workers, and has been confirmed. Support for this view comes also from a comparison of photosynthesis in green plants with photosynthesis which occurs in the some of the lowly sulphur bacteria. These bacteria produce carbohydrate in light from the raw materials carbon dioxide and hydrogen sulphide. The equation expressing this action is taken to be :

$$6\,CO_2 + 12\,H_2S = C_6H_{12}O_6 + 6\,H_2O + 12S$$

Now compare this with the normal photosynthesis of the green plant

$$6\,CO_2 + 12\,H_2O = C_6H_{12}O_6 + 6\,H_2O + 6\,O_2$$

In the first equation the sulphur is, of course, all obtained from the hydrogen sulphide. If the mechanism of ordinary photosynthesis by green plants is similar, then the oxygen must come from the water.

Although quite a lot of work has now been done on providing green plants with radioactive carbon it is not yet possible to say what are the intermediate carbon compounds between carbon dioxide and

sugar. Results by different groups of workers have appeared to give contradictory results, but quite recently Calvin, one of the workers in California, using the tracer technique, has announced that the first recognisable carbon compound to appear in photosynthesis is glycerophosphoric acid, a compound of glycerol and phosphoric acid which it has been established with a good deal of certainty, is also formed when the sugar breaks down in respiration. This is a very remarkable finding and one which gives ample scope to theorists, but we must wait until evidence is produced of other intermediate products of photosynthesis before its actual course in the green plant can be indicated with anything approaching reasonableness. It would, however, seem that the glycerophosphoric acid is formed subsequent to the energy absorbing process; this, as we have seen, involves a reduction, and actually some oxidation is involved in getting sugar from glycerol. We may note, however, that glycerol is a 3-carbon atom compound, and we must regard it as very unlikely that carbon dioxide gives rise directly to glycerol. Certainly in respiration, between the glycerophosphoric acid stage and the production of carbon dioxide there appears to be a chain of five or six reactions, and it is scarcely reasonable to suppose that the synthesis of glycerol from carbon dioxide should be appreciably simpler than its breakdown to carbon dioxide.

It is interesting to note, in view of Calvin's discovery of phosphoglyceric acid during photosynthesis, that more than 50 years ago van't Hoff suggested that the building up of sugar from carbon dioxide might in part follow the course of respiration in reverse.

What then do we really know of the mechanism by which the green plant provides us with food and energy? We are sure that the process includes stages or reactions which depend on light and stages which are independent of light, that the absorption of carbon dioxide occurs in a separate reaction from the evolution of oxygen, that the latter appears to be involved in the process in which energy is actually absorbed, that this consists in the reduction of some substance, and that the water, one of the primary reactants, provides the oxygen. At some stage glycerol in the form of glycerophosphoric acid appears, from which sugar is ultimately produced. This is not very much really, and yet much more than we knew 50 years ago, and in some important respects more than we knew even 10 years ago, but it looks as if we are a long way yet from solving the problem of how the green plant manufacures sugar, and so of being able to devise machinery for utilising solar energy in the same way as the plant. Yet with all our experience of the achievements of scientific men in other spheres it would certainly be rash to deny the possibility of this being done, or of its being done within a comparatively short time. Certainly the use of radioactive tracers should help us to find other intermediate substances formed in the process, and information of this kind would give strong clues to the actual reactions involved.

The plant physiologist who sets out to discover the mechanism of photosynthesis has fulfilled his task when he had laid bare the complex of reactions which make up that process. He is not, as a pure scientist, concerned with the economic aspects of his problem. But as an ordinary human being he is as much concerned with the economic consequences of photosynthesis as he is concerned as a man of science with discovering the mechanism of the process.

Now we have already seen that photosynthesis is the basic process which provides us with our food and with nearly all the energy required for the very numerous manufacturing processes which distinguish our modern civilisation and which provide us with so large a proportion of the amenities of life. Even if it is accepted that crop production resulting from day to day photosynthesis has produced sufficient food for human needs, and the supply of coal and mineral oil, the results of photosynthesis in the past, has been adequate for the provision of the amenities of life in our present stage of civilisation, what of the future? Few people have been so unjustly abused by the ignorant and prejudiced as Malthus for putting forward the self-evident thesis that population must be limited by the means of subsistence available, and if so far, given the means of distribution, the

total food production of the world could be made sufficient for all human needs, it is clear that if population continues to increase, the amount of food grown must increase in proportion if that population is to be as well fed as it is at present. But, even if it is still a very long way off, a limit must be reached at some time to the amount of food that can be produced in the world. The only ways we can see at present in putting off the arrival of that day are by improved methods of crop husbandry and by bringing fresh land under cultivation, and no doubt much can be done yet in these directions towards increasing the world's food supply.

Suggestions have been thrown out that much might be done by growing crops of algae in the sea. I referred earlier to the fact that photosynthesis carried out in the oceans of the world is probably several times as much as that of land plants. The small plants of the ocean, the phytoplankton, diatoms for the most part, are said to occupy a layer 300 feet deep. Moreover, the amount of carbon dioxide is much greater in the sea than in the air ; it has been estimated that the atmosphere contains about 600,000 million tons of carbon dioxide while the seas of the world contain nearly one hundred times as much. Some of this carbon dioxide is of course turned into food in the form of fish, but there is undoubtedly here a vast reserve of the raw materials of photosynthesis. To utilise the photosynthetic power of algae in this way in order to produce food and energy far in excess of what is produced at present in agriculture and forestry may appear at present a sort of Wellsian fantasy, but it should not pass the wit of man to do it and it would appear at present much less far from realisation that the imitation of the photosynthetic process completely outside the green plant. There is, however, no reason to suppose that this is not ultimately capable of accomplishment, and that following that, the synthesis of all the multifarious products manufactured in the plant and animal body may not ultimately be possible in the laboratory and factory. Whether the human race will last long enough in a civilised condition for this stage in scientific development to be reached is another matter, and one on which I certainly would not hazard a guess.

Apart from food production, the process of photosynthesis in past ages has provided us, we as have seen, with the coal and oil on which most of our industries and the amenities of civilised life to-day depend. About the only other source of energy available to-day is water power and although this is by no means negligible it cannot replace coal and oil. It was calculated by Steinmetz that if all the rain which fell on the United States was collected and all the power it could produce on its way to the sea was developed, the theoretical hydro-electric power so produced would be about the same as that obtained from coal consumption over the same area. But since only a small proportion of the total theoretical hydro-electric power could actually be used it is obvious that water power could never be able to replace coal and oil.

There remain then, as far as we can see, two ways of solving the problem of obtaining energy when coal and oil supplies are exhausted. One is, of course, the utilisation of atomic energy, and it may be that this will provide the solution of the problem. The other is to utilise the method of the green plant in converting the energy provided in a constant supply from the sun, either by more intensive growing of plants in the ways I have already mentioned, or by imitating outside the plant the process of photosynthesis. How far we are towards being able to do that, I have tried to indicate. It may be noted that even then, if it is correct that solar radiation results from the conversion of hydrogen to helium, we shall be using atomic energy, that of the hydrogen bomb. Indeed this is what we are doing, and what has been done from the beginning of life on this planet, in the provision of food. But however much atomic research may render coal and oil unnecessary as a source of energy, I think we may be sure that the green plant will retain its importance as the primary converter of solar energy on which we and all living things are dependent for our existence on this planet.

A GEOLOGICAL APPROACH TO MIDLAND PREHISTORY[1]

by

PROF. F. W. SHOTTON

THE contacts between Geology and Prehistoric Archaeology are many and varied, but it is true to say that they are at their greatest when Archaeology is dealing with the most ancient part of its subject matter. Older Palaeolithic times covered a phase in earth history when topography was very different from what it is to-day, when climatic variations ranged from glacial to warm interglacial, and in the elucidation of the complex cycles of erosion and aggradation which then occurred, the ' human ' stone implement is the most valued of all aids to the stratigraphical geologist. By later prehistoric times, for example, from the Neolithic onwards, the face of Britain had attained a relief very similar to that which we now know, with its rivers following modern lines and flowing in valleys with the same sort of slopes that we see to-day. The geological story being virtually finished, archaeology is no longer the valuable servant of geology which it was during the Palaeolithic period ; but the converse is not true. It is the purpose of this article to illustrate two aspects of post-Palaeolithic prehistory where a geological approach is useful, and to use the Midland counties as the demonstration ground. These two subjects are :

(1) The geological pattern of a region and its effect, directly or indirectly, on the ease of human movement and so upon trade and migration routes.

(2) The specialist examination of stone implements as petrological specimens, with the idea of tracing them to their places of manufacture and so building up a pattern of dispersal trails from these centres.

[1] Substance of a paper read to the Anthropology and Archaeology Section at Birmingham on September 1, 1950.

The Geological Background to Midland Prehistory

It is important to point out that in the Midland counties there is no Chalk and no Carboniferous Limestone, while very little of the Cotswold Jurassic limestones can legitimately be included in the area. The area thus lacks those formations which elsewhere provide continuous tracts of sparsely vegetated land so suitable for the movement of peoples. It is not, then, surprising that the Midlands have come to be regarded as a barren district to the prehistoric archaeologist and though this is a wrong impression, the fact will always remain that central England will never produce a richness of finds comparable with that of the Chalk downs.

If one takes into account only the geological map of the Midlands which shows ' solid ' formations, one could, however, obtain an erroneous impression of the nature of the country. The Lias clays, the great extent of Keuper Marl, the Coal Measures of Warwickshire, Staffordshire and Shropshire, the Old Red Sandstone outcrops around the Clees (and the Old Red Sandstone, despite its name, has a large amount of clay in its make-up) and the shale valleys which separate the Edges of east Shropshire, all combine to give an impression of the dominance of heavy clay land. The major exceptions to this generalisation would be the high bleak grit moorland of the Longmynd and the areas of Worcestershire and Staffordshire where the lower part of the Trias, i.e., the Bunter and to a less important extent, the Keuper Sandstones, outcrop.

This picture is, however, considerably modified when we take into consideration the Pleistocene (drift or superficial) deposits. It is difficult for anyone unfamiliar with the Midlands to do this. A

considerable part of the region is not covered by modern maps of the Geological Survey showing drift, and although the blanks are partly covered by the publications of such people as Prof. L. J. Wills and Dr. M. E. Tomlinson, there still remain appreciable parts of the area where no published drift maps are obtainable.

The superficial deposits, existing now as disconnected patches of once much more continuous sheets, comprise mainly boulder clays, lake clays, outwash sands and gravels and river terrace gravels. Their present distribution profoundly alters the picture of soils obtained from a first inspection of ' solid ' maps. The boulder clays and lake clays can rest upon Trias sandstones and give heavy ill-drained land where sandy heath might otherwise have been expected ; conversely, the sands and gravels can provide belts of well drained soil upon the great spread of Keuper Marl. Notable examples of this second effect are :

(*a*) the long and very narrow line of gravels which caps the Ridgeway, forming a connection between the sands of the Bromsgrove area and the Avon River terraces north of Harvington ;

(*b*) the large gravel plateau in east Warwickshire which still carries the names of Dunsmore, Ryton, Bourton, Lawford and Wolston Heaths and which controls the distribution of roads and settlements ;

(*c*) most important of all, the almost continuous line of gravel terraces which fringe the rivers Avon and Trent and important lengths of the Severn.

The geology of an area can exercise two very obvious controls on scenery—by affecting its relief and by influencing the vegetation. Both these effects could have important results in determining the spread of people across the country, but in the Midland area it can hardly be claimed that relief is often strong enough to form a physical barrier to movement. Perhaps only in east Shropshire (with its Wenlock Edge and View Edge of Silurian limestones), in the steep hills of Uriconian volcanic rocks (Pontesford, Wrekin, Lawley, Caradoc) and at the Malverns can we picture actual ground slope as being a real hindrance to movement. The more subtle effect of geology in influencing soil and drainage and so determining the natural vegetation, must have had a more important effect on the distribution of people. If we construct a map showing the occurrence of heavy clay soils, in contrast with the sand and gravel terrains, it is immediately apparent that the great forests of Wyre, Arden and Needwood lie (or lay, for two at least are much diminished by man's clearing activities) upon the clay lands. It is also very striking that, if we plot the distribution of the small patches of woodland which still exist, however modified by forestry, in a county such as Warwickshire we find them still to be almost wholly on the clay soils.

I would not like to over-emphasise the correlation between geology and the evidence of prehistoric man in the Midlands. Not all sandy soils are well drained —if they are low-lying, they can and do give rise to swamps—and though the clay lands can be thought of as heavily wooded where they were not forming bogs in the valley bottoms, these forests would not be wholly impenetrable. Nevertheless, if we superimpose on our soil map the places where post-Palaeolithic, pre-Roman implements have been found (see, for example, the map by H. Thorp and L. F. Chitty, Fig. 18 of ' Birmingham in its Regional Setting,' though I am here considering a rather wider area) there are some obvious correlations between geology and distribution. Statistically there is clearly a preponderance of finds on the sandy soils. One notes the area between the Severn and the Ridgeway, from Bromsgrove to Pershore, an uninterrupted stretch of clay country virtually devoid of evidences of prehistoric man. Again, to the east of the Ridgway, extending to Birmingham, the River Blythe and nearly to Stratford, lies another large area of barren clay country.

In contrast to these, the valleys of the Avon and Trent show a marked concentration of implements and both were clearly important lines of movement. One would expect that river valleys would form lines of communication, but it is the fringing terrace gravels, perched as dry shelves above the rivers' floodplain, which allowed this and it is here that the implements have been mainly found. Where, in the upper reaches of the Avon system near Coventry, the terrace gravels approach

closely to spreads of earlier glacial gravels, there is a marked increase in the number of finds.

The Severn differs from the Trent and Avon exactly as it should do in the light of its different geological history. Originally the Severn above Ironbridge flowed out towards the Dee and only during a phase of the Ice Age was it diverted southward. In this process a deep gorge was cut from Ironbridge to Bewdley and in this part of the river there are no appreciable terrace deposits. Whereas in the open parts of the Severn Valley above and below the gorge there are extensive terrace spreads and an abundance of prehistoric finds, the gorge itself is almost barren. Though the valley may also have been a line of movement like the other big rivers, either the gorge was by-passed or there was little inducement to settle along it.

Away from the rivers, two other belts show a concentration of finds which cannot wholly be explained by the personal factor of an ardent investigator. The most striking of these connects the River Teme with the Severn—across the Silurian and Old Red rocks (largely sandstones) of the Clun country, the grit plateau of the Longmynd and the gravels which lead down to the Severn at Shrewsbury. The other belt is less concentrated but nevertheless, I believe, definite. A broad zone which runs from Lichfield, through Sutton Coldfield, and west of Birmingham, to Rubery, Stourbridge and Kidderminster is built up almost entirely of Triassic sandstones, sometimes covered by glacial gravels and may have formed an easy if rather ill-defined route connecting the upper Trent with the Severn. From it there could also have been relatively easy cross connection, from Sutton Coldfield via the sandstone ridges of the Warwickshire coalfield, to Coventry and the Avon line.

The Petrological Examination of Stone Axes

The idea of examining the stone from which an implement was made by cutting out a slice and converting it to a transparent section for microscope examination, was first put seriously into practice in the south-western counties of England and has led to important results which have been published. The Council for British Archaeology now plans to extend this kind of survey over the whole country and I have been personally concerned with the identification of rock types in the case of specimens catalogued by Miss L. F. Chitty and Mr. W. A. Seaby for the counties of Shropshire, Herefordshire, Worcestershire, Warwickshire and the southern half of Staffordshire.

It is important to understand just what this technique can and cannot achieve. It is a great advance on the old method of examination by eye and the lens, which has led to many errors, and it is possible to identify the nature of a rock completely in thin section. Having made these identifications, it becomes possible to group a number of implements as being all made from the same rock and therefore possibly all emanating from one point of origin. Given enough of such identifications, they could point the way, when plotted on a map, to the factory site from from which the implements came ; but for this method to yield results, a considerable number of specimens are required. It is also important that the whole country should be surveyed, for in a restricted group of counties the trails will usually lead one outside the surveyed area. It must also be remembered that some types of rocks are very widespread. We find many celts made of basalt and, try as we will to subdivide these into groupings based on differences in the size, relative abundance and degree of alteration of the minerals, we may still be left with a number of specimens which appear to be exactly the same petrologically—yet we cannot assume that they originated at a single place. Greywackes, sandstones and many volcanic ashes are equally difficult to tie down to a restricted point of origin. There is also the further complication which must be kept in mind—even when the rock of an implement can be identified with a localised outcrop, there is always the possibility that a boulder from this, having been transported far afield by a glacier, was recognised by some prehistoric man as a good axe rock and that he forthwith set up manufacture on the spot—far from where we might credit the axe as having been made.

There are, however, two sorts of cases

where petrological examination points clearly to the source of origin. One of these occurs when the rock is unique in type and we can match it at one outcrop only. The other case is that of a known axe-factory, for even if similar rock is known elsewhere, it is a reasonable assumption that an implement which *exactly* matches the rock of a factory site did in fact emanate from that spot. Emphasis must, however, be placed on the word *exactly*, for a rough similarity is not good enough and it was because of this that the old method of hand-specimen examination led to errors.

Keiller, Stuart Piggott, Wallis and Stone in their publications, for the south-west counties, set up eleven petrological groups, but I need only stress four. Their Group I, by far the most abundant, was a uralitised gabbro which they nearly, but not quite, matched in Cornwall. Their Group VI corresponded to the factory site at Langdale in the Lake District, Group VII to another factory at Graig Lwyd, Penmaenmawr, and Group IX (thinly represented) was the product of the Tievebulliagh factory in Antrim.

Before seeing how these types come through into our West Midland counties, it would be as well to indicate what total number of specimens we are dealing with. A total of 204 celts, adzes, hammers, axes and hammerstones have been catalogued. Of these, 20 hammerstones made by the simple perforation of smooth pebbles have not been sectioned, nor have the 35 flint celts. Of the balance of 149, some are lost, a few have not yet been allowed to pass into our hands, and 102 have been sectioned. This is not a very large total on which to base conclusions. Yet I have definitely recognised 5 examples of Group I (a decrease, compared with the proportion in the south-west counties, which lends credence to the postulated Cornish source), 9 specimens from Langdale, 7 from Graig Lwyd and 2 from Tievebulliagh. These numbers hardly permit us to plot clear trails or to suggest that the implements were traded along narrowly defined belts, but they are another step in what, when the national survey is completed, will be a picture of the spread of culture from a number of prehistoric ' industrial centres.'

In our Midland survey, it has also been possible to recognise two new groups of local origin. One of these is of small importance numerically though it has its interest in relation to the concentration of finds that has already been noted around Coventry. In the Cambrian rocks near Nuneaton are sills of camptonite, an unusual rock type. Five implements have been identified with this rock and it is something of a confirmation that, within the area I have surveyed, it is purely a Warwickshire group. It remains for future work to decide whether it is also in the adjoining counties of Northamptonshire and Leicestershire.

Much more important numerically is a group of no less than 22 axe-hammers made from a rock known as picrite. It is such an unusual rock that merely to match it in all its details by an outcrop would be enough to fix the place of origin (apart from the possibility of a glacial boulder). This has been done at a locality known as Cwm-mawr, on the Shropshire-Montgomery border just south of Corndon Hill. The scatter diagrams of these finds shows clearly that the site has been correctly identified, for specimens abound in Shropshire but are as yet unknown in east Worcestershire or Warwickshire. Petrological examination has thus, I believe, located an axe-factory site before its actual discovery. We already know of 4 axe-hammers of this rock in Wiltshire (2), Hampshire and Devon, so that Cwm-mawr will take its place along with Graig Lwyd, Langdale and other sites, as one of the focal points from which the spread of implements will be traced.

SOME RECENT ADVANCES IN LOCUST RESEARCH

by

DR. B. P. UVAROV, C.M.G., F.R.S.

ALTHOUGH locusts are amongst the oldest enemies of mankind, research work on the locust problem has always been intermittent and more continuous and systematic studies did not commence until some twenty years ago. At that time, a great locust plague threatened Africa and western Asia and this made several governments, including the British, realise the need for investigating not merely the best means of killing locusts, but also the reasons for their periodic swarming, resulting in periodical economic catastrophes. In this country a small section of the Imperial (now Commonwealth) Institute of Entomology was given the task of organising, collecting and analysing data on locusts in Africa and western Asia, and this led to the establishment, in 1945, of the Anti-Locust Research Centre, an institution with an extensive programme of research into all aspects of the locust problem. The French Government also created a Committee for the Study of Locust Biology; in India a special organisation was set up for research, and in Egypt a Locust Section of the Ministry of Agriculture was created; in South Africa, research at the University of Pretoria was already in progress.

The present review aims at presenting only a brief summary of recent advances in the study of the locust problem. The progress made has been due to the combined efforts of several governments, and of many institutions and persons, and it is often impossible to allot credit for an achievement to anyone in particular. An important feature of the work, especially during the period 1930–38, was a very high degree of international co-operation in locust research. That this has been achieved was due, at least partly, to the existence of the Anti-Locust Research Centre which served to stimulate co-ordinated investigations in many countries, while it also played a prominent part in the organisation of five successive international conferences. At these conferences, common problems were discussed by experts of many nations, research programmes were prepared and the work shared out. Important researches have been carried out by individual countries, amongst which India, Egypt, France, Belgium, Italy and South Africa have been particularly active.

The first point which it was necessary to clear up was, primarily, a geographical one. It had been known that tropical Africa was subject to plagues of three different species of locusts: Desert Locust (*Schistocerca gregaria* Forsk.), African Migratory Locust (*Locusta migratoria migratorioides* R. & F.) and Red Locust (*Nomadacris septemfasciata* Serv.) Most of the old records of swarms in Africa, however, referred merely to 'locusts' and it was impossible to disentangle the evidence referring to each species separately. Several international conferences agreed that all locust reports from Africa should be sent to the Anti-Locust Research Centre, with full data as to the species, stage of development, direction of swarm movement, etc., according to a standard form. This was a great step forward, since it has made it possible to plot the data on large-scale maps so as to build up a complete picture of the movements of each locust species, and to follow the developments from month to month and from year to year. Tens of thousands of reports had to be dealt with, but the results fully repaid the effort. Within some two to three years it became clear that each of the three main African locusts has its own invasion area, although they partly overlap, and in the past this has been a source of confusion.

When sufficient data had been accumulated, it was possible, not merely to follow

the developments, but to analyse them with a view to establishing the normal annual cycle of each locust in every part of its geographical area. These normal cycles proved to be correlated with local climates, and thus studies on locusts had to be closely linked up with climatology. In each climatic area the seasonal cycle of locust breeding proved to have a definite pattern, mainly dependent on rainfall, while migrations of swarms between breeding periods showed definite seasonal trends, largely connected with wind systems. This knowledge has made it possible to forecast locust breeding and movements of swarms. As a result, monthly forecasts are now regularly issued by the Anti-Locust Research Centre, and it is a notable achievement that now no country in Africa and western Asia need be in fear of an unexpected invasion—warnings of impending danger are usually given well beforehand.

When purely cartographic work had shown close dependence of locust migrations on weather, the next step was to investigate, by direct observations, which weather factors affect locust flight. Field research parties, equipped with modern meteorological instruments, have been carrying out such observations in East Africa. This work is still in progress, but there are already some valuable findings. Sunshine, for instance, proved to have a profound effect on flying activity, locusts being able to fly in sunshine at air temperatures which would otherwise immobilise them. The height at which swarms fly may reach many thousands of feet above the ground, and this is related to the intensity of thermal convection currents which develop over heated ground and provide a lift to locusts as they do to soaring birds and gliders. Winds have a particularly strong influence on horizontal displacements of swarms, leading to invasions. It has often been asked whether locusts fly with the wind, or against it. An objective study showed that there is no simple answer. Normally, a locust takes off into the wind, but its flying speed is about eleven miles per hour, and if the wind is stronger than this, the locust will be carried by it, although its own course may be a different one. In light winds, there is a tendency to fly against, or across, the wind and the general direction of displacement, i.e. of migration, will vary accordingly. All details of this balance between an actively flying locust and a moving medium of the air are not yet sufficiently known, but it is clear that the problem requires a combined investigation by biologists and meteorologists. On the biological side, the locust's responses to weather factors are of obvious importance. In this connection, an interesting discovery has recently been made of a special organ, consisting of hair patches on the head of a locust, which enables the locust to perceive air currents and stimulate it to take off into the wind. Another important physiological aspect of long-range migrations is the source of energy, and laboratory work is in progress to study the utilisation of reserve substances by suspended locusts made to fly for long periods. Parallel with this work, research is being carried out on the aerodynamics of locust flight, and the general result of all these studies should be a better understanding of details of connection between locust migrations and weather ; this will provide a sound basis for both long- and short-range forecasting of swarm movements, which is important in organising control. In any case, the problem of migrations of locust swarms, which sometimes travel for hundreds of miles, is being gradually unravelled. There is still much work to be done, but there is no longer any reason to regard migrations as something of a mystery.

Another striking feature of locust biology which has also been regarded as mysterious, is the mass marching of young locusts, or hoppers, as they are called. Bands of hoppers, consisting of millions of individuals, are known to march in dense formation, by-passing some obstacles, overcoming others and suddenly invading crops. Recent studies and experiments, both in the field and in the laboratory, have shown that marching activity of hoppers, like the flight of swarms, is affected by temperature, sunshine and wind. Marching is, to some extent, stimulated by hunger, but there is no reason to believe, as has been thought in the past, that hoppers march in search of food ; in fact, hopper bands often march away from good vegetation into the desert.

An important part in these mass movements is played by the optical responses of hoppers. Thus, the coherence of a marching band is due to visual inter-attraction of hoppers, while the unanimity of the direction of march is a result of optical compensation reaction : images of neighbouring hoppers marching forward move across the eye of a stationary hopper, and the hopper stabilises the images by moving in the same direction. A subsidiary orientating influence is provided by a reaction to the sun : it has been shown that a band marching with the sun shining on it from one side, will turn about if it is shaded and a reflection of sun is made to shine on it from the opposite side. Since marching hoppers, as we shall see later, usually have a large amount of black in their coloration, they are liable to be very strongly heated by direct sunshine, and heating of their bodies increases their marching activity. With a fall in air temperature and the sun setting, bands usually stop for the night, to resume their march next morning. Hopper behaviour in the morning is interesting as showing their dependence on radiant heat from the sun : if the morning is cool, hoppers are quite immobile, but the first rays of the sun falling on them cause some slight movement and soon clusters of hoppers are seen on the best lit spots, all sitting with their sides perpendicular to the rays of the sun—they are basking. As the sun grows hotter, the orientation of hoppers changes—now they sit facing the sun, and thus exposing to its rays the minimum surface. After this, marching begins. Thus, obervations on hopper behaviour in the field showed the need for fundamental studies on their vision, on possible thermoreceptors, etc., and these studies are in progress.

Of course, an important feature of locust biology is feeding. Their voracity is proverbial, but this is an impression based rather on their occurrence in vast numbers than on the amount of food taken by individuals, a problem which is being investigated now. It is not true, either, that locusts will eat anything ; in fact, different species of locusts have their favourite food-plants, and some plants are not eaten at all.

Apart from providing basic information on migrations and seasonal cycles, the cartographic approach to the locust problem has resulted in another important advance. When breeding areas and swarm movements of a locust species have been plotted for a whole period of its swarming, extending for several years, it was seen that a locust plague usually commences in a restricted area, spreads from it gradually to the maximum and then dies down. For instance, some small swarms of the African Migratory Locust were noticed in the flood plains of the Middle Niger near Timbuctoo in French Sudan in 1926–27. In 1928, swarms spread to Senegal and Sierra Leone ; by 1929–30, the whole of West Africa became invaded ; in 1930 the invasion spread eastwards to Anglo-Egyptian Sudan and then the plague descended into East Africa, reaching Rhodesia, Angola and South-West Africa in 1932. Thus, a small original area, some 50 × 120 miles in extent, gave rise to a plague which overran, at the lowest computation, some 8–10 million square miles and lasted for fourteen years. Of course, this spread was not continuous, but interrupted by breeding, and the spread of the plague was due to a number of successive generations of locusts.

With regard to another species, the Red Locust, a similar picture was obtained. A few small swarms were noticed in 1927–29 in the Mweru Wa Ntipa marshes in Northern Rhodesia and the Rukwa valley in south-western Tanganyika, but in 1930 they started spreading out and some 200 reports of roaming swarms were received. By 1933, the number of records rose to 900, by 1934 to 3,600, and in 1935 no less than 6,300 swarms were reported. Since in this species only one generation in a year is produced, this increase of the plague and its spread from two small areas to some 3 million square miles provided an excellent example of the rapidity with which a locust menace can grow and extend.

Cartographical studies could, of course, supply no more than a general indication of the sources of locust plagues, but they made it possible to concentrate the search for these sources. A number of entomologists of various nations—French, British, Belgian, Egyptian, Indian and South African—undertook arduous journeys to

explore the suspected areas, and investigate the conditions which make these areas particularly favourable for the formation of initial swarms. By that time it was already known that every locust may occur in two very distinct phases : the solitary and the gregarious (or swarming) and the initial search was particularly directed towards finding out the distribution of locusts in the solitary phase. Soon, however, it became clear that not all areas inhabited by solitary locusts should be regarded with suspicion, but only those where transformation into gregarious phase had been observed, or its possibility suspected from other evidence.

Laboratory work on phases provided clear indications that phase change occurs in connection with hopper density. Hoppers reared in isolation are characterised by mostly uniform coloration, very often green, but with considerable individual differences ; those reared in crowds are remarkably similar to each other, bearing a bold pattern in which black and yellow (or orange) colours, predominate. Adults of the phylum *gregaria* are less strikingly different from those of *solitaria* in coloration, except in connection with sexual maturation when they develop yellow pigmentation, particularly bright in males, whereas solitary adults show no maturation changes. On the other hand, there are considerable morphological differences between adults of the two phases ; some of these differences are so obvious, that locusts of the extreme phases have been mistaken in the past for different species, while others are discoverable by exact measurements and calculation of ratios between parts of the body, and their statistical analysis. Since locusts of one phase can be transformed into the other experimentally, it is only natural to expect that the extremes are connected by a continuous series of transitional forms.

Studies of phase variation have made good progress recently, but they mainly consisted of accumulation of biometrical data and of experiments, not always well controlled. Coloration differences between phases were subjected to thorough biochemical studies, which made it possible to determine the pigments involved, but the metabolic processess involved in striking colour changes are still awaiting a physiological study.

The laboratory evidence that phase change is connected with population density has been used in the field investigations directed towards discovery of the sources of locust plagues, or of outbreak areas as they came to be called. The question as to how the density of a population of solitary locusts can increase to the level necessary to cause phase transformation is not a simple one. A mere increase in numbers of locusts, if they were evenly distributed over a uniform area, could hardly bring about the required crowded conditions. Two factors, however, tend to assist in the process. The first is a considerable sensitivity of locusts, both hoppers and adults, to environmental conditions, resulting in movements from habitat to habitat, and this may lead to concentration. For instance, females of Desert Locust lay eggs in damp sand ; if a shower of rain is followed by a dry spell, there will soon be only some spots still remaining suitable for laying and locusts will lay in them. Thus, eggs may become somewhat localised and the hoppers that hatch a couple of weeks later will be found in more or less dense patches. These hoppers usually feed on small ephemeral plants which spring up in the desert after a shower ; they soon dry up, thus depriving hoppers not only of food, but also of shelter. Although marching of hoppers, characteristic of the gregarious phase, is not conspicuous in the solitary hoppers, the latter, nevertheless, tend to wander, particularly when stimulated by sunshine, temperature and wind. In their random wanderings, some reach denser vegetation, not yet burnt up by the sun, where the stimuli for movement are reduced. The result would be a concentration of hoppers, and it all depends on the numbers available in the area and on the relative size of the concentrating patches, whether the critical density would be reached. Hoppers in such concentrations resulting from random movements are subject to mutual influence, due possibly to visual inter-attraction which develops as a result of close association. They now tend to keep together and begin to march in groups ; small groups meeting each other often fuse and larger hopper bands are formed.

The habit of morning basking on small surfaces exposed to sun-rays also induces concentration, perhaps even at an earlier stage than wanderings induced by differential drying-up of vegetation. There are other ecological factors and seasonal processes which favour initial concentration in many different ways, but the first essential feature of outbreak areas is patchiness of the habitat, particularly with regard to vegetation ; another feature is the instability of the environment, which changes rapidly with the season.

It will be seen that studies of phase transformation in the field require a thorough understanding of the environment, on one hand, and of the locust's responses to it, on the other hand. With regard to the environment, studies are required, e.g. on microclimates in such situations as on bare sand and in a tuft of grass, in a clump of a broad-leaved plant, etc.; differences in temperature, humidity, light and wind in such closely adjoining situations are known to be strikingly great. Such work has been done in the past only in a preliminary manner, but now much more exact investigations are in progress, with the help of modern instruments for measuring microclimatic factors. Of course, the locust side of the problem requires very detailed laboratory experiments on behaviour, since there is already some evidence that a change in the behaviour of a solitary hopper can be induced by placing it in a crowd of gregarious ones, when it proceeds to march. The problems of inter-attraction and of mutual stimulation demand serious attention.

While the dynamics of locust outbreaks are presented above mainly as a result of concentration due to ecological factors, the available initial population of locusts is obviously also of importance. It is also important to be able to follow changes in density during the initial stages of an outbreak and to analyse them quantitatively. The main difficulty in this work is that of the method of estimating the number of locusts in a given uniform area. Low density and rather inactive habits of solitary hoppers make direct counting unreliable, and this is even more so in the case of adult locusts which may be wary and conspicuous in hot weather and hiding in grass when it is cool. Studies on the methods of population research are in progress, particularly in this country, where they are being carried out on British grasshoppers, relatives of locusts but living in a more stable environment and, therefore, presenting a somewhat simpler, but still a very difficult, problem.

Having covered, very briefly, the main aspects of research on locusts, let us turn our attention to the problems of their control, a sphere in which also considerable recent advances can be recorded.

Defence of standing crops against locust invasions is relatively simple from the technical point of view. Powerful new synthetic insecticides provide the means of killing locusts at a small cost and without any risk to livestock or to plants ; newer and better insecticides are continually appearing on the market. The application of insecticides is being mechanised, which makes it more economical, both in the chemicals and in labour and, although no perfect machinery exists to suit special requirements of locust control, it is only a matter of time until the necessary further improvements in the technique of locust killing are achieved.

To be able to kill locusts in a given well-defended area is, however, not the same thing as to control a plague. Swarm migrations extend over hundreds of miles and breeding areas are not the same from season to season. Swarms produced, for instance, in Arabia are a menace to the whole Middle East and to eastern Africa ; swarms produced in summer in West Africa invade North Africa and breed there in spring, their progeny flying back to the tropics. An essential condition of successful control of a widespread plague is a careful overall planning, so that control units are ready in all the right places at the right time. This involves, first of all, a knowledge of the continually changing disposition of enemy forces, and in this respect the centralisation of locust reports in one place, as described above, has been a great step forward, as this has made possible both the general planning and the forecasting of locust movements. Planning of large campaigns, as for instance against the Desert Locust whose invasion area covers half of Africa, the Middle East and the Indian sub-continent, would be useless without willing co-operation of the

many countries involved. In this respect, a great deal was achieved in the period 1943–47, when it became possible, partly because of the special war-time conditions and facilities, to co-ordinate anti-locust measures in most of the invaded countries. The campaign costs were estimated at about £1 million per annum, but this expenditure was fully justified, since this was the first time in history that a plague of Desert Locust caused practically no losses of crops. A similar campaign, in which the experience of the past is being used to introduce improvements, is now in progress against the new plague of the same locust which commenced two years ago. Again, every effort is being made to induce all countries to adopt modern control methods and to link up the local plans, which is not easy in the present disturbed state of the world. In other countries and continents it appears even more difficult to reach the necessary pooling of efforts and, as long as this is impossible, insecticidal control of locusts will remain a palliative, and a costly one at that.

Since recent investigations have shown that locust plagues arise from relatively restricted outbreak areas, it was natural to conclude that a discovery and delimitation of outbreak areas of each locust species should provide a definite hope of checking outbreaks in the initial stages. The discovery of outbreak areas of the Red and the African Migratory Locust has provided a basis for testing this theory. After necessarily protracted discussion, the groups of African countries respectively threatened by the two locusts have agreed to set up permanent organisations, internationally financed, whose first duty it was to keep a constant supervision of the outbreak areas and to suppress any concentrations or small swarms. The system proved to be workable and both organisations have already scored successes in suppressing local outbreaks which otherwise would have developed into plagues. Such suppressive control of locusts has one drawback—it must never be relaxed, and even a temporary slackening of scouting may lead to some concentrations escaping attention and getting out of control.

There is, however, a possibility, although still somewhat theoretical, of a more radical solution of the problem. Since the formation of initial swarms occurs only in special outbreak areas, these areas must possess some peculiar ecological conditions which favour gregarisation of solitary locusts. If these conditions were known, it might be possible to alter them in the direction of preventing phase transformation. There is little doubt that these conditions are not the same for different locusts, and research in each outbreak area is needed before the practical measures of outbreak prevention can be formulated. This type of research which must be continuous is an obvious long-range task of the same permanent organisations which are at present engaged in outbreak suppression, but it requires a much greater development of their research activities.

This rapid review makes it justifiable to say that locust research is not only making progress, but has already some definite achievements to its credit. On the other hand, it is only now that many of the component problems are beginning to crystallise and demand concentrated attention. It is of general interest to see how this essentially entomological problem is now claiming for its solution the combined efforts of a number of sciences : physiology, biochemistry, meteorology, geography, aerodynamics and agronomy, to mention only some of them.

One might well ask whether the present plans for a rapid agricultural development of subtropical and tropical countries do not in themselves spell the doom of locusts, as many people believe. Unfortunately, this belief has no foundation in fact, and it was the firm opinion of the international conference of locust experts in Cairo in 1936 that the available evidence tends to show that locust activities are, on the whole, favoured rather than hindered by man's activities. Deforestation, overgrazing, patchy cultivation in desert areas are all, in one way or the other, contributing to make the locust more difficult to control, while the extension of crop areas in regions liable to locust invasions tends to increase the economic risks. The locust problem as a whole will remain a problem for a long time to come, but it can be ultimately solved, if the requisite detailed knowledge about each locust species is available. This can only come as the result of much more research.

CHEMICAL ENERGY

THE Chemistry Section included in their programme for the Birmingham meeting a symposium on Chemical Energy. The papers were introduced by Prof. F. H. Garner who reviewed the sources of energy available and referred to the problems of converting the chemical energy of various fuels into heat, mechanical and electrical energy (Cf. 'Energy in the Service of Man,' *Advancement of Science*, No. 28, p. 386). Prof. Garner pointed out that the efficiency of conversion of heat energy to mechanical energy, being determined thermo-dynamically by the initial and final temperatures, was usually less than 40 per cent. and that this was the limiting factor for the efficient production of electrical energy from chemical energy via the usual stages of heat and mechanical energy. He referred to the avoidance of such high losses of energy by the production of electrical energy directly from chemical energy through the oxidation of metals in voltaic cells and stressed the advantages that a fuel cell, based on the fundamental raw material, coal, would have if its operation on a large scale could be achieved.

The texts of the four papers on particular aspects of the subject are printed below.

CHEMICAL ENERGY IN COMBUSTION REACTIONS

by

Prof. F. H. Garner, O.B.E.

IT is proposed in the present paper to consider the efficiency of the use of chemical energy from combustion in heating processes and of its conversion to mechanical and electrical energy, rather than the chemistry of the production of special fuels or of the combustion process itself.

The first step, that of combustion—the transformation of chemical energy into useful heat—can have a high efficiency where opportunities are available for using the sensible heat of the combustion gases at low temperatures, as for example in steam raising. But, where the combustion gases are discharged at relatively high temperatures, as in some metallurgical operations and the manufacture of bricks and glassware, the efficiency is relatively low.

The next step, that of transforming heat energy into mechanical or electrical energy, is usually one of relatively low efficiency, of the order of 20 to 30 per cent. even in an efficient engine or power station. However, much higher efficiencies can be attained if the heat exhausted from the engine at a low temperature level can be put to some useful service such as space heating.

Main Uses of Fuel

The principal purposes for which fuels are used may be grouped under three headings: first—space heating and cooking, both domestic and industrial; second—industrial power and heat; third—transportation.

Space Heating and Cooking.—For Great Britain this represents about half the total, with the remainder probably about equally divided between industrial heat and power and transportation. The efficiency in use is only about 20 per cent., as against 50 per cent. on the continent of Europe and 60 per cent. in the United States. For the United States, where the total consumption of fuel used per person is half as much again as in Great Britain, space heating takes only 23 per cent., industrial power and heat 45 per cent. and transportation 32 per cent. (W. M. Holaday, R. E. Albright, T. L. Apjohn and L. R. Steffens, *Proc. Amer. Pet. Inst.* 1949, 29, 111/16.) The greater use made of the mechanical power, from the chemical energy of fuels, in the United States corresponds to a higher standard of living in that human labour has available a higher proportion of mechanical energy.

Industrial Power and Heating.—The use of fuels in their natural state, that is before coal is carbonised or petroleum cracked, leads to high thermal efficiencies only in a few applications, one of the chief of which is steam raising, where thermal efficiencies of 90 per cent. are obtainable in efficient

power stations. This operation is efficient only where steam can be used directly in space heating, in drying operations or in various chemical engineering applications, as for example in the sugar industry where efficiencies as high as 80 per cent. are obtainable.

One of the most important uses of steam is conversion to electrical energy, and there is a large loss of heat in this process, which has to be dissipated in huge concrete cooling towers, and the maximum overall efficiency is not greater than 32 per cent. ; in practice this falls to about 20 per cent. by the time the electricity reaches the consumer owing to various losses including transmission. Electricity, however, represents the only form of energy suitable for some applications, and for the direct production of mechanical power it can be used at over 90 per cent. efficiency ; from the point of view of convenience and efficiency, electricity is for such purposes far more efficient than other forms of energy.

Similar considerations as regards low efficiencies apply in other industrial applications. Thus, in many furnaces at metallurgical works, thermal efficiencies of not more than 4 to 5 per cent. are obtained ; other examples are the manufacture of bricks or glass. In these industries the demand is for high-level heat and there is available large amounts of heat at lower temperature levels which can be used only by the provision of expensive capital equipment in such forms as waste heat boilers, condensers, insulation and heat exchangers.

Transportation.—The overriding factor in the utilisation of the chemical energy from fuels is, however, that of provision of the energy in a convenient form for use. Thus, as already mentioned, liquid fuels are largely used in the transportation field. In the Second World War intensive efforts were made to use solid fuels (in portable gas producers) and compressed gaseous fuels for transportation, particularly in countries such as Sweden and Germany where liquid fuels were in short supply, but the substitutes were quickly discarded as soon as liquid fuels became available. The choice of fuel is thus dictated largely by the equipment which has been developed for performing the required mechanical operation, by convenience in use, by handling and storage and ultimately by the cost to the user.

In the field of transportation, there are three forms of engine ; internal combustion engines, divided into spark-ignitio.. (petrol) engines, compression-ignition (diesel) engines, and gas turbines in various forms. For land transportation the diesel engine, as will be seen later, is thermally more efficient than the petrol engine but the latter, owing to its earlier development, has reached a more advanced stage of application, particularly in the smaller power units. The diesel engine is, however, finding increasing use in the larger size units, as in rail and sea transport, owing to its higher thermal efficiency.

The efficiency of utilisation, η, of the chemical energy in spark ignition and compression ignition engines depends on the compression ratio of the internal combustion engine.

$$\eta = 1 - \left(\frac{1}{R}\right)^{\gamma - 1}$$

where γ has the value of about $1 \cdot 27$, and R is the compression ratio of the engine.

Spark Ignition Engines

For internal combustion engines of the spark ignition type, the motor fuel naturally present in the crude petroleum is not a suitable fuel for the modern engines. Ricardo showed that unless suitable fuels were supplied for a given compression ratio, ' knocking ' developed with its attendant tendency to pre-ignition, causing loss in efficiency and possibility of damage to the engine. In order to increase the thermal efficiency by using higher compression ratios, aromatic hydrocarbons from coal tar were introduced as the first antiknock agents, but later the cracking process was developed and still later a whole series of special fuels and hydrocarbons was manufactured for engines of high compression ratios. The cracking processes were modified to give fuels of higher antiknock quality—higher octane number—but it is the cracked gases which have been largely used in the conversion by the processes of polymerisation, alkylation and isomerisation to produce synthetic hydrocarbons which have made possible the modern

aviation fuels—which are still further improved by the addition of the antiknock agent, tetraethyl lead. The earlier cracking operations were largely thermal cracking, that is cracking at high temperatures and pressures, but for the production of these special fuels the fluid catalytic process has largely taken their place. The resultant improvement in quality of motor and aviation fuels has only been achieved at the expense of chemical energy from petroleum on an increasing scale, with resultant loss in overall thermal efficiency. Thus some recent figures by Holaday (*loc. cit.*) show that an increase of 10 octane number over the present standard in the U.S.A. would require an increase in refinery fuel to the extent of 10 per cent. of the heating value of gasoline produced and at a marked increase in plant—that is in capital cost of refining equipment.

The efficiency of utilisation in the motor car of the chemical energy in motor fuel is quoted by Holaday as only 6 per cent. largely because motor vehicles are operated most of the time at part load ; at full load their efficiency is 18 per cent. This low efficiency is due to the motorist's desire for high speeds and rapid acceleration, although most of the time the engine will be producing power equivalent to less than one-quarter load, with accompanying high energy losses. This figure is very low, only slightly better than the steam locomotive for rail use, which is quoted as 4 to 5 per cent. For British motor-car engines the efficiency is higher, perhaps by 50 per cent., but no precise figures are obtainable. Owing to the higher cost of fuel in the United Kingdom, smaller more highly efficient engines have been developed which are operated at greater load capacity.

The aircraft engine, on the other hand, operates mainly at full load and reaches an efficiency load of 12 per cent.

Compression Ignition Engines

In the compression ignition engine or diesel engine, particularly with the lower boiling distillates used for high speed engines in road transport, high cetane number is important and this requires broadly the presence of hydrocarbons of high H:C ratio, as opposed to the need for low H:C ratio or highly compact molecules for high octane numbers for spark ignition engine fuels. By reason of the higher compression ratio, the diesel engine in its various forms is the most efficient of all engines for converting heat energy into mechanical or electrical energy.

An interesting comparison is that between the coal-fired steam locomotive at 4 per cent., the oil-fired at 6 per cent. and the diesel electric locomotive at 26 per cent. In the United States this development is leading in a very marked degree to a rapid replacement of the steam locomotive by the diesel. Thus, the steam locomotive which mainly uses a solid fuel, coal, is now being replaced by diesels using liquid fuels.

Whilst the compression ignition engine is more efficient than the spark ignition engine owing to its higher compression ratio, considerable engineering development is necessary before it can be applied widely to the lower horsepower range used in passenger motor cars.

In industrial power generation, it is more efficient than the steam turbine when used for converting chemical energy into electricity, the best figures being 36 per cent. for the diesel engine as against 32 per cent. for coal- or oil-fired steam generating stations.

In addition to this increased availability of chemical energy, diesel fuels are in general used in the natural state in which they exist in the crude petroleum. For the distillate diesel fuels as used in high speed engines, buses and trucks, a reasonably high cetane number is necessary, and the quantity of such fuels in available crude petroleum is limited. The marked expansion in demand which is occurring may therefore make it necessary to segregate the higher cetane portion, mainly of higher H:C ratio, from the more aromatic and naphthenic fractions by such processes as solvent liquid refining or to develop synthetic methods of manufacture at the expense of some of the chemical energy in the fuels. The larger diesel units, as for example in marine transportation, use more viscous fuels than smaller units and are not so exacting in their requirements ; but further engineering developments in diesel engine manufacture are necessary so that they can run on even more viscous fuels.

Gas Turbines

So far we have not considered the gas turbine, developed by Sir Frank Whittle, using a kerosene fuel for aviation but now being extended to larger units for power development. As fuel for such large units, residual fuels, powdered solid fuels and gas derived from other fuels are being considered.

The gas turbine apparently does not have limitations of octane or cetane number which apply to the spark ignition engine and the compression ignition engine, the fuels for which must have suitable types of hydrocarbons present. On the other hand, the gas turbine was developed on a clean distillate fuel and the use of fuels of high ash content may lead to deposits and corrosion of the turbine blades. Even petroleum residual fuels with low ash content have led to such deposits, due apparently mainly to the presence of minute vanadium contents of the order of 0·04 per cent. and even less in the fuel.

Much further engineering work is therefore necessary before it can be decided whether powdered coal or petroleum residues are practicable fuels for the gas turbine or whether they will have to be converted into gas or purified from ash. An indication of possible improvement in the efficiency of the gas turbine is given by developments under consideration in coal-burning turbine locomotives for which, if the engineering difficulties can be overcome, efficiencies of 15 to 17 per cent. are expected, three times that of the present coal-steam locomotive.

The future of the larger sized gas turbines, for which efficiencies of over 30 per cent. are expected, in competition with steam turbines and diesel engines for electricity generation, appears to depend on the extent to which these natural fuels, coal and petroleum fuels, can be employed, although gas in a turbine is in some respects more amenable to the higher efficiencies than steam.

Conclusion

Thus, in considering the greater utilisation of the chemical energy of fuels we have to bear in mind that in many applications the choice as regards the form in which the energy is to be used may be the deciding factor. As has already been pointed out, for automotive equipment, motor cars, lorries, buses, aeroplanes and tractors, there is no practical alternative to liquid fuels. The ease of handling of liquid fuels is also an important factor in marine transport.

During the war Germany had to rely on the manufacture of liquid fuels from coal by hydrogenation and from synthesis gases by the Fischer-Tropsch processes. Such processes use a large proportion of the chemical energy in the conversion process and hence cannot compete with petroleum products except under the stimulus of tariff protection. Thus, whereas in the refining of petroleum to motor fuel, diesel fuel and fuel oil, 10 per cent. of the chemical energy may be required in the refining process, for the conversion of coal to motor fuel by gasification and synthesis or by hydrogenation some 55 per cent. of the chemical energy of the fuel is required.

The thermal efficiencies at which our stored measures of fuels are being consumed are very low, over 80 per cent. being wasted. The increased productivity of man is dependent on the use of the chemical energy from fuels at an increasing rate, particularly in the form of horse-power available per man. Thus, the problem of economy and greater efficiency in the use of fuel is one of outstanding importance. The problem is now being realised and man is himself devoting more energy to the efficient use of fuel than at any previous time in history.

Chemical Energy of Reactions other than Combustion

by

Dr. James Taylor, M.B.E.

It has been shown in the previous paper how chemical energy can be utilised for performing useful work and how, in the case of conventional fuels, the oxygen required for the chemical reaction is derived from the air. There is another important class of chemical reaction which man has harnessed to replace human effort in which the fuel and oxygen are condensed together, as it were, ready to react when suitably initiated. Such reactions require no oxygen from the air. They are

diverse in chemical nature and character, but they must of course have the common property of being exothermic. They are not an economic method of providing energy, of course, since they carry their own oxygen. Thus one gram of coal takes roughly two grams of oxygen from the air for complete combustion and gives about 8,000 calories. One gram of cordite on the other hand provides only 1,000 calories. However, for providing high power for very short intervals of time, or for use under conditions when atmospheric oxygen is not available, these compositions provide simple and convenient power sources.

Many of these reactions are very violent in nature in which case they constitute high explosives. Some like cordite are slower, the rate depending on the pressure of operation, and in such cases they comprise propellants used in guns and for other purposes. Another class is almost gasless, like Thermite, and such are used as convenient sources of heat for specific purposes like welding.

Work during recent years has disclosed in addition a fascinating field of self-sustained low temperature combustion reactions in the solid state which are only now achieving important applications. Finally, there is a whole range of exothermic gas-producing liquids like hydrogen peroxide and its mixtures, methyl nitrate and other alkyl nitrates, the applications of which to rockets and guided projectiles have made great progress under the duress of war.

Explosives

The essential operative property of explosives is that they are self-propagating when initiated or ignited from a local point. In the case of high explosives 'point' initiation is normally provided by a local intense shock or blow as from a detonator, which comprises a small metal tube filled with an explosive like fulminate of mercury that detonates violently when initiated by a spit of flame. Initiation in the case of propellants like cordite or incendiary mixtures is thermal in character, generally consisting of a flame.

The earliest example of the explosive type of reaction was gunpowder, which is an intimate mixture of charcoal, sulphur and potassium nitrate. This is the parent of all the mixture class in which fuels like charcoal or cellulose react chemically with the oxygen from a nitrate, chlorate, or the like. Gunpowder was remarkable in that it enabled large quantities of energy to be released in extremely short intervals and by it spectacular destructive effects could be achieved. It was inevitably applied to the art of warfare and a long period elapsed before it was adapted to the winning of coal and stone, and to civil engineering purposes. It was not until the latter half of the nineteenth century that a further great advance occurred, namely in the development of detonating explosives like guncotton $(C_6H_7N_3O_{11})_n$, nitroglycerine $(C_3H_5(NO_3)_3)$ and T.N.T. $(C_7H_5N_3O_6)$. Gunpowder actually deflagrates or burns with the oxygen contained in its composition and it is relatively slow. The pressure which it can build up to do work is only of the order of 30–40 tons per sq. in. Detonating explosives, however, when suitably initiated, are tremendously fast and pressures of the order of 1,000 tons per sq. in. are realisable. The nature of the reaction in such cases is quite different from that of gunpowder, the chemical reaction being initiated and sustained by a mechanical shock wave which passes at high velocity through the explosive. The velocity of this shock wave, which is akin to a sound wave, is extremely high, in some cases almost 9,000 metres per second.

Some idea of the differences in the speed of the reactions can be demonstrated vividly by igniting short trains of cordite, gunpowder and lead dinitroresorcinol (L.D.N.R.). The cordite burns relatively slowly, a 4-inch length taking about 40 seconds; gunpowder flashes off in approximately one second; while the same length of L.D.N.R. is decomposed almost instantaneously with a loud report. These reactions are, as already mentioned, dependent on the pressure of operation. The reaction of L.D.N.R. is not a true detonation but serves to illustrate the very fast speed which is possible at atmospheric pressure.

Combustion reactions produce relatively little damage to nearby objects other than by burning or scorching. On the other hand, the shock wave which is set up in a

detonating explosive is capable of causing considerable damage even when the explosive is unconfined. If a charge of gunpowder is fired in contact with a steel plate no deformation of the plate occurs, while if blasting gelatine is used the plate is completely perforated.

Recent years have seen a great amount of investigation on the theory of detonation in condensed explosives and it has now been placed on a quantitative basis by the development of the hydrodynamic theory of detonation, thereby enabling the velocity of detonation, detonation pressure and the other detonation parameters to be calculated.

Detonating or high explosives are used widely, both for civil and military purposes. They are vital in the production of coal, limestone, and other minerals, and have been one of the greatest boons to man in cutting out a tremendous amount of human toil.

Gunpowder is still used for blasting, where a ' gentler ' action than that given by high explosives is required, such as in slate quarrying.

With the rapid increase in industrialisation of the country, based on coal, the need for blasting explosives of increased safety was very urgent and gunpowder never provided any real safety in coal mines because methane-air mixtures, which occur in fiery mines, can be initiated by an ordinary flame however small. With the advent of the detonating explosives, the position was completely altered and by incorporating cooling materials like common salt into high explosives based on nitroglycerine (or T.N.T.) and ammonium nitrate, the so-called ' permitted ' explosives were developed. Such cool explosives can, in fact, be fired into methane-air mixtures, or even fired suspended directly in such mixtures without causing ignitions, provided the charges are restricted in size, and they have brought about an outstanding advance in safety in coal mines.

Gunpowder was the only propellant for guns and firearms until towards the end of last century when cordite, which is a plastic made from nitrocotton and nitroglycerine, was produced and superseded it. Gunpowder was also the only filling for rockets until the last war. Many people think that jet propulsion is a very recent invention, but this is not so. Gunpowder war rockets were used well over a century ago; in fact the British Fleet bombarded Copenhagen in 1801 and Boulogne in 1806 with war rockets.

Improved rockets were introduced using cordite instead of gunpowder during the last war and they have also been used for assisted take-offs for aeroplanes and the like. As in previous instances, the industrial and commercial uses of cordite lagged behind the military ones and the first ' peaceful ' application—if it can be so termed—was for operating a humane cattle killer. This is a simple example of a mechanical operation carried out by the use of an explosive charge. The gases from a small amount of a propellent explosive are used to drive a piston (the captive bolt of the gun) down a cylinder. This rectilinear motion can be converted readily into rotary motion as is required for some purposes, and in recent years increasing attention has been given to using cordite and similar propellants as controlled energy sources for performing mechanical operations. This embraces the so-called ' power cartridge ' field and includes cartridges for starting aeroplane engines, for the emergency ejection of pilots from high speed aircraft, for operating fire extinguishers and a whole variety of purposes. The ' philosophy '—as the Americans would say—of carrying out ' non-military ' operations by exothermic solid chemical reactions has been established.

Gasless Reactions

I would now turn to other types of solid reactions which have found applications and deal first of all with reactions that are practically gasless, like Thermite, which comprises a mixture of aluminium powder and iron oxide, the reaction between the two substances producing aluminium oxide and metallic iron with a very large emission of heat which raises the mass to an extremely high temperature:

$$3Fe_3O_4 + 8Al \longrightarrow 4Al_2O_3 + 9Fe + 719 \text{ kcal.}^1$$

(920 cal./g.)

[1] It should be remarked that all the reaction equations which follow are illustrative rather than precise, since many of them are very complex.

The uses of Thermite are well known, but other rather gentler types of gasless mixtures have also found application. For example, a mixture of hammerscale and calcium silicide contained in a suitable metal cartridge was used widely during the war as an immersion heater inserted in cans of soup. The main reaction can be represented by the equation :

$$5Fe_3O_4 + 4CaSi_2 \longrightarrow 4CaSiO_3 + 4SiO_2 + 15Fe + 101 \text{ kcal.}$$

Another mixture, antimony and potassium permanganate, which is practically gasless, has found considerable application in making delay fuses for detonators. The main reaction for this mixture is :

$$2KMnO_4 + 2Sb \longrightarrow \tfrac{1}{3}[K_6Sb_2O_8] + 2MnO + \tfrac{2}{3}Sb_2O_5 + 179\text{kcal.}$$

(320 cal./g.)

For these fuses this composition is pressed into metal sleeves of varying lengths which can be inserted into detonator tubes, between the normal electric fusehead and the detonator composition. By this means, closely controlled delay times can be obtained, and a series of detonators can be arranged to explode at definite intervals following a single passage of an electric current through the connecting wires. This provides a novel means of controlled blasting, which enormously facilitates tunnelling operations for hydro-electric schemes and the like, by speeding up the blasting operations and also reducing the hazards, to the workmen, arising from frequent returns to the working face in the presence of toxic fumes and dangerous roofs.

Low-temperature Gas-producing Reactions

I made reference earlier to self-sustained low-temperature combustion reactions. In the early 1930's experiments were initiated with the object of discovering low-temperature gas-producing reactions which might be used for safety blasting in coal mines. The first self-sustained composition of suitable properties which was discovered was a molecular mixture of sodium nitrite and ammonium chloride. This is self-sustained when initiated locally, by a hot wire for example, and gives a low-temperature reaction, together with the production of nitrogen, steam and salt :

$$NaNO_2 + NH_4Cl \longrightarrow NaCl + 2H_2O + N_2 + 51 \text{ kcal.}$$

(420 cal./g., 540 c.c./g.)

It has also the remarkable property of being initiated by weak acidic substances. This mixture known as ' Hydrox Powder ' is now employed in a steel tube blasting device which is particularly suited for use in fiery coal mines.

Attention was directed soon afterwards to ammonium nitrate, which is the best and cheapest energy- and gas-producing substance used in the explosives trade. The decomposition of ammonium nitrate is a complex business and many different reactions are possible. The reactions are exothermic, but of low calorific value and some are relatively very cool. The criterion of exothermicity is not, however, enough to define a self-sustained composition and ammonium nitrate does not decompose readily. Means of catalysing its decomposition were therefore sought and finally it was established that certain substances like potassium chromate or ammonium dichromate added in quantities of the order of a few per cent. caused self-sustained decomposition of ammonium nitrate.

Such compositions are capable of being pressed into compact cylinders and will provide gas steadily for long periods, much longer than is possible with cordite, for example. They can be used for driving motors or as fuels for rockets and jet-propelled devices. They have also found application for the dispersion of pesticides, since, because of the low temperature of reaction, they are capable of vaporising considerable quantities of organic materials without decomposing them.

Later on attention was turned to guanidine nitrate, and a number of catalysts or sensitisers was found capable of promoting its decomposition in a self-sustained manner.

$$CH_6N_4O_3 \longrightarrow 2H_2O + CO + H_2 + 2N_2 + 51 \text{ kcal.}$$

(416 cal./g., 1100 c.c./g.)

These guanidine nitrate compositions have achieved a number of applications, but the one with which the public is familiar is the jet propulsion unit used for

driving toy aeroplanes, motor cars and speed boats. The pellets used in these jet engines burn in a steady cigarette fashion with constant thrust for operating the toys.

Turning finally to liquid fuels it may be observed that they have an advantage over the solid types which I have previously described in that their supply can be controlled by mechanically operated valves and the combustion providing the hot gaseous products is capable of being regulated over a wide range or even stopped altogether at will.

The best-known liquid monopropellant is concentrated hydrogen peroxide (80–85 per cent. H_2O_2). It is spontaneously decomposible into steam and oxygen when catalysed by various permanganates.

$$H_2O_2 \longrightarrow H_2O + \tfrac{1}{2}O_2 + 13 \text{ kcal.}$$
(387 cal./g.)

It was used, for example, in the launching device for the V1 and the turbine-driven pump system of the V2. Usually, however, hydrogen peroxide was used with fuels such as alcohols in order to use up the free oxygen and increase the energy release. Such systems are, of course, bipropellants and include liquid oxygen with alcohols as used in the V2, concentrated nitric acid and aniline as used in the U.S.A. aeroplane take-off units and a number of others.

Alternative monopropellants are methyl nitrate (CH_3NO_3) which is a powerful explosive and dangerous in use. Possible liquid propellants which have been described by Wheeler, Whittaker and Pike, including nitromethane (CH_3NO_2), nitroethane ($C_2H_5NO_2$), ethyl nitrate ($C_2H_5NO_3$) and a number of other organic nitro-compounds and organic nitrates.

(Dr. Taylor's lecture was illustrated by numerous demonstrations.)

Direct Conversion of Chemical Energy to Electrical Energy

by

Dr. H. J. T. Ellingham

Energy Considerations

A voltaic cell may be regarded as a device for converting the free energy of a process into electrical energy. In principle, almost any type of spontaneously occurring chemical reaction is capable of being harnessed as a voltaic cell and the chemical energy of the reaction thus rendered convertible to electrical energy in the discharge of the cell.

In practice, only a few types of chemical reaction have been effectively harnessed in this way—as primary cells. Indeed, the operation of practically all established forms of primary cell is based on the oxidation of a metal (generally zinc) to one or other of its compounds. Although the efficiency of conversion of chemical energy to electrical energy in the discharge of such cells may be high, the yield of electrical energy is normally very small in relation to the total amount of energy in various forms that has had to be expended in producing the metal by reduction of compounds derived from its ores. In the ordinary applications of primary cells, however, the over-all energy efficiency, or even the efficiency of the discharge process itself, may be of less significance than other characteristics, such as portability and the ability to deliver a specified current for limited periods at intervals during a long effective life.

If an efficient primary cell could be developed to harness the oxidation of carbon, however, such a cell might furnish an alternative means for the large-scale generation of electrical energy from coal. The possibility is attractive because the chemical energy of the fuel (plus oxygen) would then be convertible to electrical energy in a single process with no upper limit of efficiency set by the second law of thermodynamics ; whereas the established system of generation involves three energy conversions—chemical energy to heat, heat to mechanical energy, mechanical energy to electrical energy—and the second of these (the heat engine stage) has an efficiency which is strictly limited in relation to the temperatures between which it can be economically operated. On a basis of energy alone, the scope for a ' fuel cell ' is indicated by the fact that even in the best modern power-station practice the over-all energy efficiency based on coal is only about 30 per cent.

Even if it proved impracticable to harness efficiently as a primary cell the

direct oxidation of a carbonaceous fuel (coal or coke), there might still be scope for an indirect fuel cell operating on the oxidation of a material that can be produced from such fuels without undue loss of available chemical energy. Carbon monoxide, hydrogen and certain metals may be considered in this category of secondary fuels. The data below show the maximum electrical energy in kWh. obtainable at 25°, 500° and 1,000° C. by the electrochemical oxidation of 1 kg. of carbon and of equivalent quantities of the specified secondary fuels (assuming that in the production of hydrogen or the metals the carbon is completely oxidised to CO_2).

Cell reaction	Maximum electrical energy obtainable kWh. per kg. of carbon		
	25° C.	500° C.	1000°C.
(1*a*) $C + O_2 = CO_2$.	9·12	9·1	9·1
(1*b*) $C + \frac{1}{2}O_2 = CO$.	3·14	4·1	5·1
(2) $CO + \frac{1}{2}O_2 = CO_2$.	5·98	5·0	4·0
(3) $2H_2 + O_2 = 2H_2O$ (g)	10·74	9·5	8·2
(4*a*) $Sn + O_2 = SnO_2$.	11·9	9·7	7·2
(4*b*) $2Zn + O_2 = 2ZnO$	14·7	12·6	

The figure for oxidation of carbon to CO_2 is practically independent of temperature and corresponds almost exactly with the ordinary heat of combustion ; it thus affords a useful basis of comparison. At higher temperatures, however, the equilibrium $C + CO_2 \rightleftharpoons 2CO$ moves increasingly in favour of CO and, in so far as the product of the cell reaction is CO, the maximum electrical energy obtainable will tend towards the lower figure for reaction (1*b*). The balance of the energy remains available, however, in the CO and represents at least a source of heat, if not of electrical energy through the operation of a separate cell for reaction (2). This latter cell reaction, considered separately (i.e. as being supplied with CO produced by non-electrochemical means), is not, however, a very favourable source of electrical energy, for even with operation of the cell at 500° C. over 45 per cent. of the chemical energy associated with the original carbon will already have been dissipated as heat in the exothermic ' producer gas ' reaction.

On the other hand, the figures quoted for cell reactions (3) and (4) give an unduly favourable impression. The reactions whereby hydrogen or the ' baser ' metals are produced by reduction of their oxides with carbon are endothermic and, unless they are linked with some other process evolving heat, additional fuel will have to be burnt to supply this heat. If allowance is made for this additional fuel as carbon, the maximum electrical energy obtainable from the subsequent cell reactions (3), (4*a*) or (4*b*) per kg. of *total* carbon supplied is correspondingly less but at 25° C. still does not lie more than 10 per cent. below the figure (9·1 kWh.) for the direct oxidation of carbon itself ; thus indicating that most of the chemical energy of the whole of the carbon used in the reduction process would be transferred to the secondary fuel if there were no heat or material losses. The extent to which the energy transfer is not quite complete even under ideal conditions depends on differences between values of the available chemical energy ($-\Delta G$) and the corresponding ordinary heat of reaction ($-\Delta H$) and will increase with rise in temperatures as the two quantities diverge.

Where the reaction for the production of the secondary fuel has to be conducted at a relatively high temperature, the carbon will be oxidised at least in part to CO instead of CO_2 and its available chemical energy will be correspondingly divided between the CO and the secondary fuel. The electrical energy obtainable from the latter will then be less (only half if only CO were formed), though here again the possibility of utilising separately the chemical energy of the CO is not excluded. Hydrogen and CO might even be electrochemically oxidised together in a single cell, but for a metal and CO separate cells would certainly be required if the chemical energy of both were to be recovered as electrical energy—a complication which it is desirable to avoid. For this reason, metals such as tin or lead which can be obtained by carbon reduction at relatively low temperatures would be preferred to higher melting metals or those with much greater affinity for oxygen. Metals such as aluminium and magnesium which

cannot be readily produced by carbon reduction of their oxides and are normally obtained by electrolysis (consuming electrical energy) are clearly ruled out. In any event, the higher the temperature of the reduction process the greater is the difficulty of keeping down heat losses, and, where a distillation operation is involved (as for zinc), heat and material losses would almost certainly bring down the over-all efficiency of generation of electrical energy to an uneconomic level.

From these considerations, it appears that the best prospects for a satisfactory fuel cell lie in the harnessing of reactions (1) or (3), or of an example of (4) such as (4*a*) involving an easily fusible metal whose oxide can be reduced with carbon at a relatively low temperature ; reaction (2) provides another but less attractive possibility. Voltaic cells based on such reactions—especially (1), (2) and (3)—have been investigated (for general surveys of earlier work, see *The Principles of Applied Electrochemistry*, A. J. Allmand and H. J. T. Ellingham, 1923 ; E. Baur and J. Tobler, *Z. Elektrochem.*, 1933, **39**, 169). Less attention has been given in this connection to the possible utilisation of reactions such as (4*a*), the advantages of which have been stressed by E. K. Rideal and U. R. Evans (*Trans. Farad. Soc.*, 1922, **17**, 466).

Experimental Fuel Cells

In order to harness any chemical reaction as a voltaic cell the reacting substances must be kept apart from one another and employed as, or fed continuously to, two independent electrodes (consisting of conductors of the electronic class) between which there is a conducting path including one or more electrolytes (conductors of the electrolytic class). For a fuel cell, therefore, the general form is :

$$\underset{-}{\text{fuel}} \mid \text{electrolyte} \mid \underset{+}{\text{oxygen}}$$

The oxygen would preferably be supplied as air to avoid the cost of separation ; the electrical energy obtainable per kg. of carbon is thereby decreased by less than 0·04 kWh. even at 1,000° C. During the discharge of such a cell, oxygen ionises at the positive electrode (cathode) yielding oxygen ions or some other type of oxygenated ions (hydroxyl ions in aqueous solutions), while at the anode *either* the fuel substance ionises (hydrogen or the metals yielding the corresponding positive ions) *or* the oxygen or oxygenated ions in the electrolyte are discharged forming nascent oxygen which then reacts at the anode surface with the fuel substance. In either case the net chemical result for the cell as a whole is the oxidation of the fuel, and the E.M.F. of the cell at any temperature is directly proportional to the free energy (chemical energy) per g.-equivalent of chemical change at that temperature.

Neither carbon nor carbon monoxide furnish corresponding ions, and their employment in a fuel cell therefore depends on their being oxidised anodically by nascent oxygen—a reaction which does not proceed at a reasonable rate except at elevated temperatures. On the other hand, the metals ionise readily even at ordinary temperatures, and hydrogen adsorbed on the surface of suitable metals (e.g. platinum or nickel) will do likewise. Such electrode systems could therefore be used in cells with aqueous electrolytes. Unfortunately, oxygen does not furnish a reversible electrode system under these conditions, and, although it ionises at the surface of suitable metals (e.g. nickel), the rate at which it does so at ordinary temperatures limits the effectiveness of fuel cells with aqueous electrolytes.

Much can be done to increase rates of ionisation and of other electrode reactions at ordinary temperature by the use of finely divided metals, either as surface coatings (e.g. platinised platinum) or disseminated through or constituting the body of a ' diffusion electrode,' e.g. finely divided platinum in carbon (Schmid's hydrogen diffusion electrode), electrodes of porous nickel (being investigated as hydrogen and oxygen diffusion electrodes under the auspices of the British Electrical and Allied Industries Research Association). Raising the temperature is of course beneficial through increasing reaction rates, but, for operation above the boiling point of solutions used as electrolytes, pressures greater than atmospheric become necessary. The free energy of the cell reaction is raised slightly by increasing the pressure (by 0·1 kWh. per kg. of carbon by raising the pressure to 50 atm. at

200° C.), but against this must be set the cost of the energy used in compressing the gases supplied to the cell. Moreover, the highly catalytic electrode surfaces that would no doubt still be needed at such moderately elevated temperatures are likely to be very susceptible to poisoning by impurities in the gases, and hydrogen obtained by the water-gas reaction would presumably have to be rigorously purified (at appreciable cost) in order to maintain the operation of the cell at high efficiency.

With all electrode reactions, in which a gaseous substance is consumed at a satisfactorily high rate, the general problem is to maintain an adequate concentration of that substance on or in the surface layers of the electrode where it is being consumed, and at the same time to provide effective contact between the electrode surface and the electrolyte. With increasing rate of consumption of the gaseous substance per unit area of electrode surface (i.e. with increasing current density taken from the cell) the effective concentration (pressure) of the substance that can be maintained at that surface under given conditions will tend to fall, and this fall may eventually become catastrophic as the maximum rate of access of the gas to the electrode surface under those conditions is approached. Any fall in effective concentration will be accompanied by a change in potential of the electrode—that of a positive electrode becoming less positive, that of a negative electrode less negative—and the working voltage of the cell, and hence the yield of electrical energy, will decrease as a result of this ' polarisation.' There is scope for improvement in electrode design, particularly perhaps in the development of new types of diffusion electrodes, for raising the current density that can be taken from a gas electrode without serious polarisation. Attempts have been made, however, to avoid ' gas polarisation ' altogether by adopting a simple oxidation-reduction system, such as ceric-cerous salt solutions, at both electrodes and maintaining the two halves of the solution (which must be separated by a diaphragm) in relatively oxidised and reduced conditions by passing air through the one and the fuel gas through the other. Such schemes have the advantage of permitting an independent selection of the electrode reactions but they present problems of their own. The function of cerium as an example of an oxygen carrier serves to illustrate the general nature of fuel cells as oxygen concentration cells ; the oxygen pressure is kept near to 1 atm. at one electrode by the supply of oxygen (or to 0·21 atm. by air) and down to a very low value at the other electrode by the fuel substance.

The fact that raising the temperature increases rates of reaction and diffusion, makes the prospects of fuel cells with molten salt electrolytes (at temperatures of 500° C. and upwards) seem generally attractive. Higher current densities should be possible without serious polarisation and there should be no need to depend on the maintenance of highly catalytic surfaces, sensitive to poisoning. Moreover, conductivities of molten salts being generally somewhat greater than those of aqueous solutions, losses due to internal resistance may be reduced. On the other hand, the maximum electrical energy obtainable from most reactions falls off appreciably with rise in temperature (see table above). Also, steps must be taken to prevent undue heat losses (e.g. by using outgoing gases to pre-heat those entering the cell) ; though it should be noted that any of the available chemical energy that is not converted into electrical energy in the cell (owing to polarisation and resistance losses) will appear as heat and thus help to maintain the operating temperature. More serious difficulties may arise through the corrosive and disintegrating action of molten salts at high temperatures on structural components of the cells. These are problems which industrial users of molten salts have no doubt largely solved, and it has seemed to the present writer that, if there is any prospect at all for high-temperature fuel cells, the producers of aluminium should be in the best position to develop these cells and should have the greatest interest in doing so, in view of their being among the largest users of direct current.

In such fuel cells a variety of electrolytes have been tried, especially molten carbonates, silicates, borates and hydroxides of the alkali metals, and various low-melting mixtures of such salts. Melts containing oxides of metals with two or more stable valency states to act as oxygen

carriers have also been investigated, as in the recent exploratory work carried out at the British Coal Utilisation Research Association by J. H. McKee and A. M. Adams (*Fuel*, xxviii, No. 1).

As an oxygen electrode at temperatures of 1,000° C. or more, molten silver is almost ideal as it dissolves notable amounts of oxygen and facilitates its ionisation. At lower temperatures metal oxides with sufficiently high oxygen dissociation pressures have been used with advantage, e.g. Fe_3O_4 at 800–900° C.

Cells for the direct oxidation of carbon have been investigated, especially by E. Baur and collaborators, e.g.

C | molten sodium silicate or borate | (molten silver), O_2

at 1,000° C. For continuous operation it would seem necessary, however, to supply carbon of reasonable purity and appropriate physical form. The use of coal itself would be likely to cause difficulties, and material such as gas carbon would certainly be too expensive ; carbonaceous material fed in as a self-baking electrode of the Söderberg type might have possibilities.

Hydrogen oxidation cells have been investigated in some detail. Thus cells of the type :

H_2, (Fe) | molten sodium and potassium carbonates | (Fe_3O_4), O_2 (air)

at 800–900° C., developed by Baur, gave a promising performance, using magnesia diaphragms to prevent undue ' wetting ' of the granular electrode materials by the electrolyte while providing adequate electrical contact between them. These diaphragms were notably attacked by the electrolyte, however, and their use would in any event add to the internal resistance of the cells. There is scope here for improved designs, involving perhaps the employment of more modern types of refractories and of electrode materials of different nature and form. Cells of this type have the advantage of being capable of operating on carbon monoxide as well as hydrogen and might even be fed with mixtures of these substances derived from water-gas.

Recently published E.M.F. measurements (Rose, Davis and Ellingham, *Trans. Farad. Soc.*, 1948, **44**, 154) on cells of the type :

Sn (molten) | SnO_2 in molten sodium hydroxide | (Ag or Pt), O_2

point to the possibility of such a system being used as an indirect fuel cell, if metallic tin could be periodically or continuously recovered from the electrolyte by carbon reduction.

There is no doubt that fuel cells of various types with molten salt electrolytes can be designed to convert a substantial proportion (certainly over 30 per cent.) of the chemical energy of the oxidation of carbon to electrical energy. Current densities in continuous operation are limited by polarisation and resistance factors, which may be seriously affected by the build-up of impurities in the electrolyte. The higher the current density that can be taken from a cell without serious polarisation, the smaller is the area of electrode surface to be provided—and, in general, the smaller the volume of the cell—for a given output of electrical energy ; raising the permissible current density is therefore an important object of design. There are many advantages in raising the temperature of operation but, if this is carried too far, they are eventually outweighed by reduction in available energy, increased heat and material losses and higher maintenance costs. In this connection, more attention might be given to the possibility of linking cell reactions consuming secondary fuels with the corresponding processes for production of the fuel from carbon. If these two operations could be conducted at the same temperature and in direct physical association with one another, the attainment of improved heat economy in continuous operation should be facilitated and the handling of materials simplified.

No satisfactory solution of the fuel cell problem is immediately in sight—but the possibility of finding such a solution should not be ruled out.

The Building-up of Chemical Energy

by

Prof. F. G. Gregory, F.R.S.

All the energy of organic life is eventually derived from the reduction of carbon

dioxide by water. The fundamental equation of this process may be written

$$6CO_2 + 6H_2O = C_6H_{12}O_6 + 6O_2 - 676{,}800 \text{ cal.}$$

The energy required for the reaction is in some cases derived from oxidation of preformed organic substances or of inorganic materials, but in the green plants the energy is derived from sunlight. The scale on which this process of photosynthesis occurs in nature may be gauged from various estimates made of the turnover of matter and energy in nature. The first such estimate was made in 1840 by Liebig. Assuming a yield of 5 metric tons per hectare (10^6 g. per 10^4 sq. m.) based upon the yield of grasslands he calculated that plants utilise 10^{11} tons of CO_2 per year, and produce 3×10^{10} tons of organic carbon. All the CO_2 of the air would be completely exhausted in 21–22 years!

A more recent estimate by Schroeder (1919) is summarised in the following table:

	Estimated area in sq. km. × 10^6.	Millions of tons of fixed carbon.
Woodland .	44	11,000
Farmland .	27	4,000
Steppes .	31	11,000
Deserts .	47	200
Total .	149	16,300 ($1{\cdot}6 \times 10^{10}$ tons)

In addition must be included organic carbon fixed by seaweeds and floating unicellular plankton. The computed value for this is 15×10^{10} tons. The fixed carbon is all derived from the CO_2 of the air and the carbonate and bicarbonate of the sea. The ocean is estimated to contain 5×10^{13} tons of carbon, the air (up to 11 km.) 6×10^{11} tons. For assimilation of plants this total carbon reserve would suffice only for 300–400 years. The supply is not in fact exhausted because a cycle of carbon exists: by bacterial activity during decay of dead organisms and the respiratory processes of plants and animals during life the CO_2 is returned to the air and sea.

We may approach the matter also from the estimated energy supply. The total energy supply from the sun amounts to $1{\cdot}25 \times 10^{24}$ cal. per annum, of which 40 per cent. reaches the earth's surface, giving 5×10^{23} cal. Of this, 50 per cent. is in the infra-red, and not available for photosynthesis, and 20 per cent. is absorbed by soil or reflected from the surface of the sea, so that not more than 2×10^{23} cal. are available for plants of land and sea.

From the equation already given for each gram of carbon fixed approximately 1×10^4 cal. are required, and thus the total energy available would suffice to fix 2×10^{13} tons of organic carbon. As already stated, the estimated total carbon fixed by all vegetation is of the order of 17×10^{10} tons. The efficiency of utilisation of solar energy must therefore be quite low—of the order of 1 per cent.

This value for the solar energy presupposes that all the light is utilised by the plant, but a considerable portion of the light is reflected from the leaf surface. Moreover the chlorophyll which acts as photocatalyst absorbs only a portion of the spectrum, and we may take as a fair estimate of the total energy actually absorbed by the pigments $1{\cdot}5 \times 10^{23}$ cal.; so that the efficiency of light energy utilisation will be of the order of 2 per cent., or of total solar energy of the order of 1 per cent.

These estimates of utilisation of solar energy are admittedly very rough, but indicate the low proportion of the light energy which can be stored as available chemical energy in plant products.

The Factors Controlling Photosynthesis

The rate of assimilation of CO_2 by green leaves is controlled by external and internal factors. Of the external factors the most important are CO_2 concentration, temperature and light.

CO_2 concentration.—The normal concentration of CO_2 in the air is around 0·03 per cent. Under conditions of high light intensity and temperatures the concentration of CO_2 acts as a limiting factor, and by increasing the CO_2 concentration the rate of assimilation can be increased considerably. Thus the leaves of the sunflower in the open air under favourable

summer conditions can assimilate 19 mg. CO_2 per sq. dm. per hr. : the leaf of the potato in full light at a temperature of 20° C. assimilates 19·2 mg. CO_2 per sq. dm. per hr. With 5 per cent. CO_2 the rate in the sunflower rises to 80 mg. and with 1 per cent. CO_2 in the potato to 81 mg. per sq. dm. per hr.

These figures are interesting in showing that two very dissimilar plants assimilate at the same rate under the same conditions. A 30-fold increase in CO_2 concentration (0·03–1 per cent.) has increased assimilation only 4-fold : this is a tribute to the structural features of the leaf which ensure rapid diffusion of CO_2 into the tissues.

Temperature.—Under conditions of adequate CO_2 supply and high light the temperature coefficient of assimilation is of the order, $Q_{10} = 2 \cdot 5$, up to an optimum temperature of 25° C. Above this the assimilation rate tends to fall in time, so that, although temporarily at temperatures above 25° C. assimilation is increased, it cannot be maintained at this high level and falls the more rapidly the higher the temperature.

Light.—Two aspects of light are important, viz. intensity and total amount. Direct estimations of CO_2 uptake by lucerne plants have shown that on cloudless days the rate of assimilation closely follows the light intensity, reaching a maximum at midday. Reduction of light intensity by clouds leads to a fall in CO_2 assimilation. Assimilation reaches a maximum at a light intensity of approximately 50 per cent. of the maximal sunlight (at 75 cal./dm.^{2}/min.).

Evidently the total amount assimilated by a leaf will thus depend upon the length of the day as well as upon light intensity, and in consequence of this the total amount assimilated varies little with latitude.

Growth Analysis

In considering the utilisation of solar energy the important matter is the amount produced by unit area of land of material which can be used for energy recovery. Since the amount of energy falling on a unit area is limited, the maximum possible yield will be predetermined by the efficiency of utilisation of the light available. The question of the efficiency of photosynthesis is thus crucial. The question arises whether plants vary in this respect as between species and varieties ; and also whether efficiency can be increased by manurial and other cultural treatments. The hope is often expressed of breeding plants with higher efficiency, and we shall have to consider the limitations of realising such hopes.

A solution of the problem of the evaluation of the efficiency of a plant as an energy storer can be achieved by simple means. The plant can be grown under standardised conditions, which can of course be varied over a wide range of external factors, and by a method of sampling the dry matter accumulation over convenient periods of time can be estimated. If over an interval of time the changing area of the leaf surface can be studied the mean leaf area can be ascertained. If then the increase in weight of the plant over the same period is known the assimilation rate can be accurately determined in terms of weight increase per unit area per unit time. In this way the net assimilation rate can be determined, and if the necessary data of the environment are collected the way is open to a thorough analysis of the growth process. The result obtained will be in terms of dry matter increase per unit leaf area per unit time. Such analyses have been carried out very thoroughly for some plants such as barley and cotton. The interesting result is obtained that for these plants growing in very different environments and at different levels of nutrition the net assimilation rate varies only within narrow limits.

Making some plausible assumptions as to the nature of the material accumulated, and correcting for variation in ash constituents and protein content, the net assimilation can be stated in terms of CO_2 assimilated per unit area and time. The mean value is of the order of 8 mg. CO_2 per sq. dm. per hr. of light.

The analysis can be carried further. By burning the dry material in a bomb calorimeter the energy content can be directly ascertained. By recording with a Callendar recorder the varying intensity of radiation which has fallen on the plants in the intervals between taking samples, and knowing also the mean leaf area, the total radiation received is known, and by comparison with the calorific values of the

products the efficiency of the energy storage can be directly determined. Such data obtained for barley during the first seven weeks of growth in 1921, 1922 and 1923 showed the efficiency to be of a low order, never exceeding 1·5 per cent., and this was true for varying levels of manuring so long as a favourable manurial balance was maintained.

If plants are grown at varying levels of nutrition the size of the plants varies greatly, but their efficiency as energy storers is constant, and so also is their net assimilation rate. It appears therefore that neither by varying the plant nor the fertility of the soil can the intrinsic efficiency of energy accumulation be greatly changed. Naturally the total quantity of dry matter, and thus of stored energy, can be increased, but this is due to the more rapid development of the plants, their greater branching, larger leaves, and greater total leaf surface.

The conditions for growth of plants as energy accumulators must be kept as near optimal as possible. There is no time to develop this aspect of the problem here : suffice it to say that in the last resort the limitation will be imposed by the factors CO_2 and total light.

Limitations of the Utilisation of Solar Energy

Bearing in mind the considerations put forward above, the question of the most effective method of utilising plant growth for storing solar energy may now be examined.

Evidently the aim should be to maintain as close a cover of the ground by vegetation as possible. In temperate regions owing to the low temperature during the winter season very little growth if any can proceed. In tropical regions of high rainfall or with irrigation no such temperature limitation occurs, but as already stated the short days almost exactly compensate for the longer growing season and total growth made is not much different.

From the practical standpoint the nature of the plant product is of first rate importance. To recover the accumulated energy it must be in a form readily convertible by fermentation processes to alcohol. The most convenient product would thus be either sugar or starch, preferably in the form of stored products in tubers or storage roots—as in the potato, sugar beet, mangold, or in tropical regions the yam. The whole question of the relative amounts of utilisable products of carbon assimilation arises at this point, and here breeding and selection of suitable varieties will no doubt be of the greatest value. The most favourable conditions for carrying out such a project would in my opinion be the desert regions, where sunlight is maximal and by suitable irrigation water supply could be maintained. Even here the low night temperatures may raise great difficulties. Tropical regions with high rainfall would appear to be promising, but such regions are covered by rain forest and much labour and expense would be required for clearing, and even then many unforeseen difficulties with disease and weed infestation may arise.

Before closing a word should be said about projects to use micro-organisms such as the green unicellular alga *Chlorella*. The advantage of using such organisms is their very great power of multiplication ; but to achieve this high temperatures are necessary and a liberal supply of nutrients. One advantage of their use is the possibility of increasing the supply of CO_2 by the use of bicarbonate. The harvesting of such minute organisms would not be a simple matter, though a continual filtration method could easily be devised—at a cost. A further difficulty would arise from the recovery of the energy from such a source. It appears that the advantages of the use of micro-organisms could only be secured by growing them in vast shallow artificially heated tanks, provided with circulating devices and filter beds. On the whole the use of higher plants with storage organs would appear more economical. The whole question bristles with practical difficulties which there is no time to discuss.

The aim of this paper has been to point out the natural limitations of the use of solar energy through the agency of plants, and to sound a note of warning against the too ready optimism of enthusiasts.

THE WORLD'S FOOD SUPPLIES[1]

by

DR. K. G. FENELON

THE volume of food produced in the world to-day is now nearly the same as that produced before the war, even allowing for the increase in world population. This is due to the progress that has accompanied the general rehabilitation of war-devastated areas and to the continued high level of food production in certain countries whose territories were not affected by war operations.

In 1948 unusually good weather throughout the world and excellent harvests in North America, enabled crop production *per caput* to attain pre-war levels, but in 1949 crop production declined to about 3 per cent. below pre-war, though it was considerably above that of 1947. The main declines in 1949 as compared with 1948 were in coarse grains and potatoes; the decline in the former being mainly due to a fall in United States production from the previous year's record level. There was, however, an increase in world meat and milk production and the output of oils and fats was slightly above the pre-war averages. In the Far East, excluding China, food production in 1949 was somewhat higher than in the previous year, especially in the deficit areas.

Much has been written about the physical shortage of food, and many authorities have expressed themselves as being profoundly pessimistic about the ability of the world to feed itself in view of the continued expansion of world population. It is important, however, to keep in perspective the significance of the word 'shortage.' Relative shortage in one country might well be relative abundance in another because standards vary so greatly throughout the world.

Progress in improving the diets of the worst fed countries obviously cannot be geared forthwith to the highest standards of the western world. Any such gearing would create a massive theoretical world shortage of food. A realistic approach to the problem involves rather the gradual raising of standards through the elementary aids to greater food production which are now being planned by governments in under-developed areas and through the assistance promised by the more advanced countries through F.A.O. or the 'Fourth Point' Programme of President Truman.

Looking back over the past hundred years, there has been a striking development in the living standards of the advanced countries which between them contain something like one-quarter of the world's population. Until comparatively recently, the peoples of most countries lived on a precarious margin of subsistence as is shown by the periodical occurrence of dearths and famines.

But though, in these advanced countries, the ordinary man can to-day buy more and better food than his parents or grandparents, the other three-quarters of the world's population has been little affected by these changes, and the people in the majority of the under-developed countries live no better than did their own ill-nourished ancestors.

Coincident with these variations in diet, standards of agricultural efficiency vary immensely, and the output of food per worker is sometimes ten times greater in an advanced country than in more backward lands. The comparison was put vividly in a Survey by F.A.O. some three or four years ago by showing that in the advanced countries one farm family feeds itself and four other families at a comparatively high nutritional level, whereas in many of the less-developed countries, one farm family manages to produce only enough to feed itself and half of another family at a much inferior standard.

So low is the present level of productivity in many countries that relatively simple improvements such as better hand

[1] Paper read to Section F (Economics) at Birmingham on September 1, 1950.

tools, better seed, crop rotations, some fertilisers and insecticides and simple pumps for irrigation would enable production to be increased by 10 to 20 per cent. or even 50 per cent. in a comparatively short time.

A feature of the world's food supply which is not always fully appreciated in this country is the fact that most countries produce the bulk of their own food. Only a very few countries, notably the United Kingdom, Belgium, Holland, Switzerland and Norway, import more than a third of their total food supply. For many commodities, the United Kingdom was in prewar days by far the largest buyer and took no less than 40 per cent. of the world's total trade in food and feeding stuffs. Western Europe, a part from the United Kingdom, took a further 30 per cent.; some European countries being heavy buyers of specific commodities such as oilseeds and maize.

National Diets.—The three main staple cereals of the world are rice, wheat and maize. Of these rice is quantitatively the most important, being the staple food of more than half the human race. Rice provides a high calorie yield per acre, and it is therefore grown, where possible, in preference to other crops where the amount of arable land available per head of the population is small. In general, the better the food standard of a people, the lower is the percentage of the total calories derived from cereals or other foods rich in carbohydrates.

National diets are determined by many factors among which incomes, natural resources, climate, tradition and habits are important. For the world as a whole, income is the most significant factor in determining a nation's diet, and as long ago as 1876 the relation between wages and diet was discussed in some detail by F. A. Walker in his book entitled *The Wages Question.* In general, prosperous nations fare well nutritionally, poor countries fare badly.

Natural resources and climate may greatly influence the diet of a people. Thus the inhabitants of Argentina, Uruguay and Paraguay are great meat eaters, whereas those of Iceland eat little meat but much fish. The environmental factor in diet is often significant and serves to explain what are often regarded as the peculiar dishes of certain countries such as bamboo shoots in China, the sea-weed foods of Japan or the ' edible earths ' of Africa. Readers of Hilaire Belloc will recall in this connection his mock lamentation:—

> ' Alas ! What various tastes in food
> Divide the human brotherhood.'

Within each country also there are often marked regional differences. Just as in Great Britain, Wiltshire has its lardy cakes, the Severn Valley its potted lampreys and Durham its singing hinnies, so in other countries there are many regional specialities.

The term ' food,' it is obvious, covers a multitude of commodities, and it is therefore essential for comparative purposes to adopt some common denominator for measuring food supplies. Unfortunately, in this regard we are somewhat in the position of the vulgar little boy on Margate Pier who ' had no little handkerchief to wipe his little nose,' because there is no entirely satisfactory single denominator, and we have to make do with the calorie. Its main limitations are that it measures only the energy value of food and takes no account of the proteins, fats, minerals or vitamins, or of qualities such as variety, palatability and all that serves to make food interesting and civilised. Food, like dress, can be adequate in a purely functional sense, but woefully drab and uninteresting. Something more than calories is required for a really satisfactory meal as the Rev. Sydney Smith fully realised when he defined the *summum bonum* as ' eating pâté de foie gras to the sound of trumpets.'

The calorie's usefulness as a common denominator is also somewhat diminished by the fact that small differences occur through the use of different conversion factors. F.A.O. has adopted new factors which differ from those used in the British White Papers on Consumption Levels and in the Economic Survey, giving a somewhat higher calorie figure than the British, though of course the latter are fully comparable between one year and another. The F.A.O. factors have been specially designed for purposes of international comparison and for this reason are used in this paper.

TABLE I.

Consumption Levels, 1948–9.

Calories per person per day.

Over 3,000	2,500–3000	2,000–2,500	Below 2,000
Argentina	Austria	Chile	Algeria
Australia	Belgium	Egypt	Burma
Canada	Cuba	Greece	Ceylon
Denmark	Czecho-slovakia	Italy	India
Eire	Finland	Mexico	Japan
New Zealand	France	Peru	Morocco
Norway	Nether-lands	Portugal	Tunisia
Sweden	Poland	Spain	
United Kingdom	Switzerland	Thailand	
U.S.A.	Turkey		
	Union of S. Africa		
	Uruguay		
	W. Germany		

F.A.O. now publishes food balance sheets for most countries but the results are still tentative, and the method needs further elaboration and refinement before the figures can be regarded as conclusive. This is especially true of countries where the statistical coverage is of a rudimentary nature. As complete accuracy is at present unattainable, and detailed figures would be misleading, Table I has been compiled from F.A.O. data to give a general picture of comparative consumption levels.

From what has already been said, it will be obvious that we cannot discuss the world food problem as though it were a uniform single problem. It is necessary to break down the problem region by region.

TABLE II.

World Food Production.

Million metric tons.

	Pre-war	1948	1949
Bread grains	148·3	162·1	158·4
Coarse grains	196·5	245·1	228·8
Rice (paddy)	149·4	147·4	147·1
Sugar	26·1	30·4	30·7
Potatoes	158·8	164·9	143·3
Fats and oils	23·4	24·4	24·5

Notes.—Fats and oils include vegetable, animal and marine. U.S.S.R. excluded.

Europe.—Let us first consider Europe. The European continent considered as a whole is the most intensely farmed large region of the world. This is the result of a combination of factors, including rich soils and favourable climate, population pressure and technical efficiency. There are, however, wide variations in types of farming, productivity and standards of living. Output per acre in North-West Europe for example is nearly three times as high as in South-East Europe. Mixed livestock farming is characteristic of Western European farming, whereas in East and South-East Europe the dominant feature is the extensive cultivation of grain crops.

During the first half of the nineteenth century, Western Europe was virtually self-sufficient in food supplies. Though population was growing rapidly, food

TABLE III.

Food Production by regions. Million metric tons.

	Far East		N. America		Europe		Lat. America		Africa		Oceania		Near East	
	Pre-war	1949	Pre-war	1949	Pre-war	1949	Pre-war	1949	Pre-war	1949	Pre-war	1949	Pre-war	1949
Bread grains	34·8	34·4	27·8	41·9	61·4	58·1	8·9	8·4	2·5	2·8	4·4	6·0	8·5	6·8
Coarse grains	28·8	28·8	78·5	118·1	54·8	49·7	19·8	15·8	6·8	8·6	0·8	1·3	7·0	6·5
Rice (paddy)	142·4	136·0	1·0	1·8	1·1	1·0	2·0	4·4	1·5	1·8	0·1	0·1	1·3	2·0
Sugar	7·3	6·4	1·7	2·0	6·5	6·6	7·6	12·0	1·0	1·3	1·8	2·0	0·2	0·4
Potatoes	7·9	9·8	11·9	13·4	134·7	113·3	2·9	4·4	0·6	0·8	0·5	0·7	0·3	0·9
Fats and oils (vegetable)	8·6	8·1	1·2	2·5	1·0	1·3	1·1	1·0	1·4	1·6	0·2	0·2	0·3	0·4

production was also increasing and millions of acres of waste land were brought into cultivation. The area under fallow was substantially reduced and crop yields were raised two- or three-fold as a result of seed selection, improvements in soil fertility brought about by the introduction of clover and root crops and later by the use of artificial fertilisers. Improved breeds of livestock were introduced and the output of milk and meat was almost doubled. By 1870 self-sufficiency began to give way to increasing imports especially from the New World which were facilitated by improvements in transport. Between 1880 and 1930 Germany's food imports increased threefold, those of Belgium, Denmark, Switzerland and Great Britain more than doubled, and those of France increased by more than 50 per cent., despite greater home production.

TABLE IV.

	Per cent. increase 1880–1930		Per cent. self-sufficient 1939 (3)
	Output of Cereals (1)	Livestock Numbers (2)	
Denmark	60	134	105
Netherlands	39	74	72
Switzerland	n.a.	43	52
Germany	44	19	87
Belgium	29	27	51
France	0	19	86

(1) Total of wheat, rye, barley and oats. (2) In livestock units, Cattle = 1, Pigs = 0·2, Sheep = 0·1. (3) Exports offset against imports. Fodder imports reckoned at calorie value of the livestock products they produce.

Before the second world war, continental Europe on its pre-war boundaries was just over 90 per cent. self-sufficient in food supplies compared with only 30 per cent. in the U.K. Within the continental area there were sizeable movements of foodstuffs as, for example, grain and animals from East and South-East Europe to the West or sugar from Czechoslovakia. There was also considerable international trading in European meat, dairy products, fruit and vegetables. On balance, Europe was a net importer of food and feeding stuffs, amounting in bread grains to 9½ million tons, of which 5½ million tons went to the U.K., over 10 million tons of coarse grains (4 million tons to the U.K.), over 2 million tons of sugar (1¾ million tons U.K.), and some 7 million tons of oilseeds (1½ million tons U.K.).

During the years between the two world wars both crop and livestock production continued to expand, but these fell considerably during the second world war, except in the U.K. where there was a considerable increase in grain, potato and sugar beet production. A gradual recovery has taken place in continental Europe since the end of the war, and by 1948 grain production had reached 90 per cent. of pre-war, potatoes 100 per cent. and sugar 101 per cent. European cattle numbers are now almost 90 per cent. of their pre-war level, though pigs are only 70 per cent.

The real problem of European farming is to be found not in the West, but in the non-industrial countries of Eastern Europe. These countries have never adopted the intensive farming methods of Western Europe, and though in pre-war days they were considerable exporters of grain and other foodstuffs, the bulk of their populations lived in extreme poverty and overcrowding. These territories have now entered the Russian orbit, and considerable internal changes have taken place. The industrialisation which is proceeding may eventually provide a solution of the problem of over-population of the rural areas, but the low productivity of agriculture is bound to continue until the cultivators acquire a knowledge of modern farming techniques. Poland, where almost all the land is still in private hands, may become an important exporter of food once the war-time devastation is overcome and production is restored to what it was before the war.

Total European population rose from 370 million in 1928 to nearly 400 million in 1938. It is now only some 390 million as a result of heavy war casualties and post-war boundary changes, though in certain countries there has been a high rate of population increase. In the Netherlands population has risen from 8·6 million before the war to 10 million now; in Italy from 43 million to 47½

million, while in Germany to-day 70 million people are living where formerly there were only 60½ million. Poland on the other hand has to-day only 24 million as compared with 34 million pre-war and Czechoslovakia 12½ million in place of 14½ million.

North America.—In contrast to Europe, land in North America is more plentiful and population densities are much lower. Farming is less intensive and efficiency, at any rate as measured by crop yields, is lower than in such European countries as the U.K. and Denmark. Should need arise, more intensive cultivation would permit very substantial increases in production to be effected. Broadly speaking, the U.S. is self-sufficient in food production, while Canada produces a large exportable surplus. During the war, food production was raised to a high level especially in the U.S. and this was maintained until 1948. The official index of the yield of 53 crops in the U.S. rose from 96·4 in 1930 (1923–32 = 100) to a record level of 137·2 in 1948, but fell in 1949 to 130. Increased production has been obtained by improved methods of crop and animal husbandry, enabling larger output per acre to be obtained. Canadian wheat production rose from a long-term average of 8 million long tons to a peak of 15 million in 1942, but since then a downward trend has been evident owing to less favourable weather. During the war the U.S. became a net exporter, but is not likely to continue so and the most recent trade returns show diminished exports particularly in grains.

Canadian food exports consist of grains (especially wheat), fresh and canned fruits, fish, cheese and processed milk. During the war, exports of eggs and bacon also became important. Post-war developments have upset the normal pattern of trade whereby Canada's favourable balance of trade with the U.K. was used to liquidate her indebtedness to the U.S.A. This is now prevented by inconvertibility of sterling and Canadian trade policy has had to be substantially modified.

The problem of surpluses is unquestionably the main future problem for North American agriculture. The U.S. Agricultural Act, 1949, reintroduced acreage allotments and marketing quotas which had been largely abandoned because of war requirements. Support prices are to be provided after 1950 on a sliding scale and increasing emphasis is likely to be placed on livestock production, involving some reduction in grain and oilseeds. Marshall Aid at present enables European countries to absorb U.S. farm surpluses, but when this ceases the U.S. will have to decide on a policy to meet the situation which would result from the loss of an important outlet for agricultural products. Several courses of action are open to the U.S. Administration. The first would be a withdrawal from world markets, but this would involve restriction of production, and if it took place before production elsewhere in the world had increased it would involve shortage and high prices for importing countries. A return to free international trading with abolition of price support and production restrictions, and the release of U.S. agricultural surpluses upon world markets at competitive prices, is not a very likely policy in view of U.S. experience in the depression of the 1930's. Other possible courses of action are the introduction of some international agency which would absorb and dispense surplus food production, or the donation of surpluses to needy countries while maintaining home prices. Among schemes of donating surpluses now being canvassed is one for giving supplies to those Asiatic countries which undertake to raise their low standards of cultivation and productivity, but none of the schemes so far put forward seems to safeguard the legitimate interest of importing countries in obtaining commercial supplies at prices which do not distort their terms of trade. The question of surpluses has, however, recently taken on a new aspect as a result of the rearmament drive and the promise of assistance to the Atlantic Pact countries. Population trends have been upwards in both Canada and the U.S. during the past 20 years, and there has at the same time been migration from rural to urban areas. U.S. population was 122 million in 1922 but rose to 149 million in 1949, while Canada's population rose in the same period from 10 million to nearly 14 million.

South America.—The countries of South America are traditionally exporters of many primary products, notably meat

and coffee, but they are deficit countries for those foods which require intensive production methods or skills beyond that of the average peasant producer. Argentina ranks with Canada as one of the two most important surplus food producing areas of the world. Costs of agricultural production, however, have been rising steeply during the past 20 years without any proportionate increase in productivity. Mechanisation on a large scale could have transformed Argentina's agriculture, but the Government monopoly has diverted the profits of agricultural production to industrial and other developments. It has been estimated that at least 400,000 tractors could have been usefully employed on the present cultivated area.

The Argentine exportable surplus of meat is an important item in U.K. supplies, but the U.K. is becoming increasingly independent of Argentine produce. Meat production seems in danger of declining like that of wheat and maize, the exports of which in 1948 were only half as large as those made in 1934. Areas sown to linseed have also declined, though rye, barley and rice have shown an upward trend. Within the Argentine, consumption of meat has risen greatly as a result of the policy of favouring industry at the expense of agriculture. Future trends, however, are difficult to forecast since Argentine Government policy is changeable, and much has recently been promised by the Government to the agricultural industry.

Beef and mutton are produced for the U.K. in Uruguay to the extent roughly of 10,000 tons per annum. There is generally no export of wheat as the country is mainly pastoral. Since the war there has been a trend towards more intensive farming and pasture is now being ploughed up for sowing to wheat and linseed.

Brazil is the world's greatest producer of coffee, and has tended to dominate world prices in that commodity. The long-continued decline in the Brazilian coffee harvest from nearly 1½ million tons pre-war to under 1 million tons in 1949 has led to a great rise in world prices of coffee. There is likely on the other hand to be a considerable increase in cocoa bean production during the next few years in Brazil, and this is an important development in view of the threat of the swollen shoot disease in the Gold Coast plantations. Brazil imports wheat and wheaten flour, but in addition to coffee and cocoa, exports maize, rice, bran, sugar, meat and fruit.

Argentina, with 14 persons to the square mile, is under-populated, though her population increased by 4 million between 1933 and 1947. Uruguay, on the other hand, is fairly densely populated, carrying over 2 million people on 3½ million acres of cultivated land. Wool export dominates her economy, and her economic future is largely linked to the world demand for wool. Brazil, though containing nearly half the total population of South America, is still a ' frontier ' farming country, and only 4 per cent. of her enormous area—equal to the size of Europe without Russia—is under cultivation.

Australia and New Zealand.—Australia, like Brazil, is a ' frontier ' farming country, and increased agricultural production is bound up with the provision of roads and water supplies. The greater part of her agricultural production is for export, but growing industrialisation and increasing population have led to reductions in the proportion of production exported. Before the war, 70 per cent. of the wheat crop was exported, whereas in 1947–8 only 43 per cent. went abroad. Australia's contribution to world food supplies will be a considerable factor during the next decade or so, but beyond that period it appears doubtful if there will be any large exportable surplus. During the last 20 years, population has risen by over 2½ million, and continuation of immigration at 250,000 a year would bring the population to 12 million by 1960. Development schemes for the large-scale raising of beef in the Northern Territories and of pork in Queensland are being undertaken, but it is as yet impossible to estimate how far these schemes will be successful in increasing exportable surpluses.

New Zealand is one of the most important food suppliers to world markets. Her small population of about 2 million provides a large surplus of food which in contrast to Australia's is unlikely to be diminished in the near future by increased demands from the home market. Her

livestock producing industry of about ½ million tons of meat and ¼ million tons of butter and cheese has always been remarkably stable. More than 95 per cent. of the major commodities were exported before the war, and though the proportion dropped during hostilities, when New Zealand was supplying the Allied Forces in the Pacific and there was an acute shortage of refrigerated tonnage, production has increased steadily during the post-war period, and a rather larger proportion than pre-war is now exported.

The Middle East.—In the Middle East agricultural methods are in general very primitive, and a deep-rooted tendency to cling to extremely complicated customs of land tenure causes severe hindrance to efforts to improve farming efficiency. Meat production receives scant attention as cattle are kept partly as draught animals, partly for their milk and hides and partly as in Africa as an outward sign of their owners' social status. The Middle East is a net exporter of barley (chiefly from Iraq) and of rice (mainly from Egypt). Syria and Turkey are wheat exporters, but some wheat and maize is imported into Egypt, Lebanon and Israel. The Middle East is a natural granary, lying wholly in the wheat belt and there is no apparent danger that the area will become a deficit area as regards food supplies. Most of the grain-growing areas need irrigation, and the costliness of such schemes may hold up further expansion. Current plans to utilise the waters of the Upper Euphrates should, if they materialise, enable North-East Syria to become one of the richest wheat-producing areas of the Middle East. The under-developed countries of the Middle East can thus expand cultivation to feed their growing populations, but the 'older' countries like Egypt look like becoming net importers. There is a vast potential awaiting development in the day of need.

Africa.—Africa is a sparsely populated continent, and as far as the natives are concerned, a low income region. Broadly speaking, there are two kinds of agricultural production—(i) Cereals and root crops, which are mainly for local consumption and form the mainstay of the natives' diet, and (ii) export crops, including oilseeds, sugar, coffee, cocoa, citrus fruits, tobacco, cotton and sisal. Despite wartime developments, native African dietary standards remain among the lowest in the world. As a whole, Africa is a comparatively empty continent with great potentialities for development, but taken as a whole it is the least well documented continent and only a fragmentary picture of the food and agricultural situation is possible.

In the Union of South Africa, wheat production doubled in the 10 years before the war ; at the same time maize production increased by about 55 per cent., but is liable to be severely affected in drought years. Meat was exported before the war from the Union and Southern Rhodesia, but Union meat exports have now ceased, and those from Rhodesia are likely to disappear before long. The chief reason is increased consumption per person and this is also the case with sugar and wheat. The Union now needs to import 40 per cent. of its wheat and flour requirements, whereas before the war imports were only needed when the harvests were bad. Union maize exports were resumed for the first time since the war out of the record 1948 crop, but a consistent export surplus is unlikely.

India and the Far East.—This region is predominantly rice-eating, and may be roughly divided into surplus and deficit rice areas. The former comprises Burma, French Indo-China and Thailand. The deficit countries are India, Ceylon, Malaya, Singapore, British North Borneo, Sarawak, Hong Kong, Indonesia, the Philippines and Japan, all of which normally import some part of their rice supplies (China and Pakistan are not normally large rice importers). Korea and Formosa in pre-war years exported substantial quantities of rice (almost entirely to Japan) and Korea but for the war would probably have resumed exports to Japan. Large quantities of other grains, particularly wheat, barley and millets are produced in India, Pakistan, China and Japan.

All the rice-exporting countries suffered severely as a result of the war and their combined production fell from about 14 million tons of milled rice in 1925 and 17½ million tons in 1938 to a little over 9 million tons in 1945. By 1948 production had risen to 13 million tons, but only

Thailand had reached the pre-war level of output. Little information is available about the production of other food crops in the Far East such as roots and other vegetables. These are, however, relatively less important, since grains of all kinds provide from 60 to 70 per cent. of the total calorie intake in most of the countries.

Generally speaking, the Far East with the exception of Japan and China is one of the most backward and inefficient farming areas of the world. About 21·6 cwt. of milled rice are produced per acre in Japan and 14·6 in China, but in India and Pakistan only 6·6 cwt., in British North Borneo 3·4 cwt. and in the surplus producing areas of Burma, Thailand and French Indo-China only 6 to 7 cwt. an acre are produced. Japan's wheat yield before the war was 15 cwt. an acre, but since the war, largely because of lack of fertilisers, it has only been 9 cwt. In China, the yield is about 9 cwt. per acre and in India and Pakistan only 5½ cwt.

The explanation of the low yields obtained in most countries of Asia is to be found in the small size of farms—three-quarters are of 3 acres or less—and the heavy rents which are charged. Cultivators are too poor to purchase even the most modest equipment, and primitive tools are employed in much the same way as has been done for centuries. In pre-war years, the rice-importing countries of Asia obtained all their requirements from other countries in Asia, but since the war rice has had to be imported from South America, the U.S., Australia and Egypt. At times, wheat, maize and sorghums have been imported owing to world shortage of rice. Wheat has had to be imported into Pakistan during the last year or so, though before the war this area exported wheat.

If the Far East—the most crowded region in the world—is to achieve similar standards of living to those of the western hemisphere, it must almost inevitably import food from some of the less densely populated continents. To do so, countries of the Far East need to produce export goods on a sufficient scale to finance the desired food imports, and at the present time they are finding this very difficult. Malaya with rubber and tin exports, and Indonesia and the Philippines with copra and palm oils can fairly readily obtain their necessary food imports, especially as their needs are small relative to those of other Far Eastern countries.

India in recent years has required some 4 million tons of imported grain annually, but her government had hoped to be self-sufficient in food by 1951. This aim has not been achieved. Nevertheless, India produced nearly 1 million extra tons of food in 1949–50 as compared with the previous year, mainly as a result of extended irrigation from numerous small works such as dams, channels and tube wells. Over 60,000 such wells have been sunk in the last year. India wisely is proceeding on the lines of numerous smaller schemes of improvement pursued simultaneously instead of embarking on grandiose projects often so impressive on paper, but disappointing in practice. There is still a great deal to be done and in the coming year India will need to import 2 million tons of foodstuffs, if famine is to be avoided.

Japan is also faced with the problem of paying for large food imports, and is likely to seek markets in Far Eastern countries for her textiles and other goods in the near future, for Japan—like Britain—must export or die.

The rate of population growth in the Far East (except for China) is among the highest in the world. In India and Pakistan, population increased by 83 million between 1921 and 1941, and is at present growing by over 5 million annually. Japan with 80 million inhabitants now has 10 million more than in 1939 and there is an additional 1¼ million to be fed each year. China is the only large country in the area where a similar trend is not apparent, for in China population growth has been checked by famine, disease and war. In the Far East, generally, populations are increasing rapidly, but agriculture is very backward and is not keeping pace.

U.S.S.R.—Before the first world war, Russia was a major producer and exporter of cereals; wheat exports averaging 4½ million tons per annum. Between the two wars, although production was expanded, domestic consumption increased and grain exports fell. Wheat exports amounted to only 0·6 million tons per annum in the 1934–8 period.

Since the end of the first world war the structure of Russian agriculture has been transformed. Large estates and primitive small holdings have given place to an economy based on large-scale State and Collective Farms equipped with a good supply of modern equipment. The process of transformation was slow, and it was not until 1929 that agriculture was integrated into the national economic plans. By 1938 some 26 million former small peasant holdings had been replaced by 3,960 State Farms and 242,000 Collectives, which between them contained 96 per cent. of the sown area. Mechanisation had by then made rapid progress and production of grains is estimated to have averaged 93 million tons. In addition, 10 million tons of grain were then being produced in those areas which have since been incorporated within the boundaries of the U.S.S.R. There has certainly been an increase in total food production since the introduction of collectivisation, but this has so far done little more than keep pace with the increase in population and urbanisation, especially as Russian agriculture suffered severely from the effects of the late war. Adequate statistical data is not available, and it is therefore difficult to assess the position in the U.S.S.R.

Conclusion.—The world food problem is neither homogeneous nor static. From the brief survey of the main food regions of the world which we have attempted, it will be seen that there are great extremes in consumption levels as well as great differences in the efficiency of agricultural production. In those countries which have been long settled there is a marked contrast between Western Europe, with its highly efficient agriculture, its industry and extensive trade, and Far Eastern countries which have remained largely untouched by advances in European and American technology both agricultural and industrial. The East, however, is now stirring, and inevitably will demand higher food standards.

To improve the food supply of the world, the greatest step forward which could be taken would be to bring the efficiency of food production in the under-developed countries more nearly up to that of the more efficient. It is significant that the Food and Agriculture Committee of O.E.E.C. in its Interim Report issued at the end of 1949 was concerned with ways of improving the technical efficiency of European agriculture. Even European agriculture, immeasurably more efficient as it is than that of so many countries in the Far East, offers great scope for increased productivity. This Committee indeed estimated that the productivity of the 25 million hectares of Western Europe's grassland could be increased by 40 per cent., apart from improvements in haymaking and silage. It is also necessary to clear the way for an expanded world trade in agricultural products so that the surpluses of one part of the world may become available for deficit areas. The first and most important step, however, remains that of increasing agricultural efficiency in the under-developed countries themselves.

THE CARRYING OUT OF RESEARCH IN SCHOOLS[1]

by

DR. A. G. LOWNDES

THIS paper refers to research carried out by members of the staff at school and not to any research carried out by the pupils themselves.

Research in the past

At one time quite an appreciable amount of research was done in schools and at least three schoolmasters attained Fellowship of the Royal Society for such research. These were Shenstone of Clifton, Meyrick of Marlborough and Baker of Dulwich. Shenstone was famous for his great skill in the making of quartz vessels, and he carried out a great deal of this work while teaching at Clifton, but Edward Meyrick did just the opposite. He took a classical degree at Cambridge, and was employed by Marlborough to teach classics to one of the forms in the Lower School. Later on he was responsible for the producing of the Marlborough College Natural History Report, but his fame was due to his life's study of the micro-lepidoptera and this work could not by any imagination come into the category of school teaching.

Meyrick certainly made a most wonderful appeal to several boys at the school, and to mention only three, Prof. Stanley Gardiner, Sir Arthur Hill and Maxwell Lefroy, all of whom attained Fellowship of the Royal Society, owed their start to him. They all came to him of their own accord because they were anxious and willing to learn. As a professional teacher of science in a school a man is by no means always able to choose his own pupils, nor are they all anxious to come to him, nor are they all desperately anxious to learn.

H. B. Baker of Dulwich was probably the most remarkable science teacher of all time. He was employed to teach chemistry at Dulwich and it was at Dulwich that he made his remarkable contribution to chemistry. To quote from the Royal Society obituary notice: 'With characteristic patience Baker spent eleven years of the Dulwich period in unsuccessful attempts to isolate two elements from tellurium.'

I never had the honour of meeting Baker myself but we are honoured with the presence of the President himself (Sir Harold Hartley), who is one of Baker's most distinguished pupils, so I feel it hardly necessary for me to say more.

In 1921 I was appointed to take on, with very little knowledge and still less experience, the teaching of sixth-form biology at Marlborough. Obviously this was a time when science masters were desperately scarce and biologists hardly existent.

At this time the attitude of headmasters and of head science masters towards research was anything but friendly. In fact it was research which led to my undoing in the first post I applied for after the 1914–18 War, which was at a large public school in the Midlands. I told the head of the science there that I wanted to continue with research. He was quite honest about the matter and told me that in that case he considered me to be an unsuitable candidate for the post. 'We want a hard-working colleague who is capable and willing to teach the elementary work of his subject. You cannot do this and research.'

At Marlborough I was perhaps wiser but less honest. I said nothing about doing research till I got there. Then the fat was really in the fire. Dr. Cyril Norwood, later Principal of St. John's College, Oxford, said quite definitely (1) you cannot possibly undertake research without neglecting your school teaching.

[1] Shortened text of a paper read to the Education Section at Birmingham on September 6, 1950.

(2) You cannot do any research which will be of the slightest value to science at a school such as this. (3) You are far too old to begin doing this type of research now.

The result was that I resigned my post at Marlborough but it ended in a compromise. I shall always regard Sir Cyril Norwood as the greatest headmaster I have ever served under. After 5 years he completely changed his opinion, and in public acknowledged his mistake. His final words were: 'A band of enthusiasts lead by an enthusiast and producing really remarkable results.' No sixth-form master could desire more. Thus ended that controversy and it ended in mutual respect.

Some headmasters seem to see true greatness in those men who are at heart musicians but are still willing to eke out a living by teaching elementary chemistry. Or again there is often a particular sort of glamour attached to the parson who can put B.Sc. or even Ph.D. after his name.

I have even heard it stated that the teacher who can wear some particular tie indicating great prowess on the football or cricket field makes a very special appeal to his pupils. My experience is just the opposite, at any rate in any first class school.

The most infectious disease among boys is that of enthusiasm. They know at once if a man has his real soul in his teaching and if this is present he needs no secondary string such as a blue or an international cap.

In this discussion I have in mind those men who are at heart scientists, first and last. As such they will look upon the teaching of science to all and sundry as their life's work, and it is by this, and this alone, that they intend to make their contribution, or render true service to the best of their ability.

What research can be done

The branch of research which claimed most of my attention was biology and I specialised for many years in the distribution of the freshwater crustacean Cyclops and the hydrogen ion concentration of the water in which it occurred. I also carried out a prolonged series of breeding experiments.

The simplest type of research for a schoolmaster to undertake is taxonomy and here there is a vast field awaiting him. Many years ago Prof. Orton wrote to me from Liverpool: 'You cannot tackle any branch of taxonomy and stick to it for seven or eight years without important results coming in.'

Taxonomy is not the ideal subject for a schoolmaster, however, for it is very limited in its appeal to the sixth-form student. The ideal is some branch which incorporates biology, chemistry and physics with a little bit of mathematics included. However, there is a vast field for the schoolmaster to tackle in almost any branch of science within reason.

If the schoolmaster is going to undertake research, the research should be primarily to increase his efficiency as a teacher of his own particular subject, and I consider that a schoolmaster's research must be dictated by his branch of teaching.

I cannot confidently discuss research in either chemistry or physics at schools but it so happened that when I left taxonomy for the time being and took up one of the more experimental sides of biology, I found myself suddenly engulfed in a subject which incorporated both physics and chemistry as well as biology. I refer to the displacement method of weighing living aquatic organisms.

This involved a critical study of the purely physical concept of density. For this work alone I had to procure some of the finest chemical balances obtainable both from this country and from Germany, and it also involved a considerable amount of correspondence with the National Physical Laboratory. On the chemical side it involved correspondence with Prof. Maurice Stacey, Professor of Organic Chemistry of this University of Birmingham. The point I wish to stress here is that it is of great advantage, *in school teaching*, if one can choose a branch of research which is not isolated.

Perhaps when working in a university it may be essential to work along a narrow field. One can certainly progress much further and faster by doing so, but this counts for little in schools. Students in the higher forms at schools are far too ready to feel that, when they have once taken up biology, they are in a position to turn their backs on anything connected

with either physics or chemistry. This is a most disastrous attitude to be adopted, either by teacher or student, and my experience has shown me that the posts available for the biologist, with no knowledge of anything outside Natural History, are becoming more and more rare and difficult to obtain. I feel that teachers taking the higher forms in schools should stress this point.

Research done actually at the school

In 1938 I retired from Marlborough and went as a private research worker or a visitor to the Marine Biological Station at Plymouth, where I have seen quite a number of teachers from schools doing research on some problem during the vacations.

I have no wish to belittle such research done away from the schools, yet I feel that those who confine their research to the vacations, and those who have to go away to do their research, may be missing perhaps a very great opportunity with their own pupils.

Students naturally have a certain amount of respect for the academic qualifications of their teachers, and it is a great thing for them to realise that their teacher is still a student like themselves, but they will gain far more if they have the opportunity of seeing their teacher actually at work, and my own experience was that it paid me considerably, on occasions, to discuss my research problems with my boys.

There is another vital point which research carried out in the actual school laboratory must bring out, and that is the fallacy that research is a thing which can only be done at such institutions as the Cavendish Laboratory, or at places where nothing but the most costly apparatus is ever seen.

A wave passing up a flagellum was first photographed, not at Cambridge, but at Marlborough. The myth that the egg of the dogfish consists of an enormous amount of *yolk* was not exposed and exploded at a university. The initial work was done at Rugby school during ordinary class teaching.

Facilities for research

Under this heading we will consider both the facilities for research and the obligation on the school authorities to provide such facilities.

Schools are not research institutions and I sincerely trust that they will never become so, and here they must differ fundamentally from the universities. They are under no obligation whatsoever to provide facilities for research. I believe that a man in charge of the higher teaching in any branch of science will increase his efficiency as a teacher enormously by undertaking research, and if this is so, then any efficient school will realise that it is to its advantage to encourage research, but I feel the initiative must come from the teacher and not from the school.

One can visualise such a state of affairs where schools receive government grants in order to become research institutions, and the salaries of the staff are supplemented in this way so as to attract men who are ready to undertake research. One may even go further and visualise schools as institutions in which the professors at the universities might plant their Ph.D. men to complete their third year, or to write up their theses for their higher degrees.

I may be setting up here a mere bogy and something which is never likely to exist. If this is so, well and good. I most sincerely hope that organised research of this type will never find its way into any of our public schools at any rate.

Surely when a man joins the staff of a school and is detailed to take the higher work, his primary duty is to teach those entrusted to him. To get the best out of every single individual pupil no matter what his ability or his prospects as a potential scholar.

I deprecate most strongly the modern tendency, though advocated by a good many, by which men with a first-class degree are paid higher salaries than men with second-class degrees in order that they shall concentrate on specialised and exclusive sixth-form teaching.

Let a headmaster, with experience, choose his own staff and leave it to him to get the best out of that staff. If some assistant masters prove themselves to be men capable of stimulating their sixth-form pupils by doing research, then is the time to give them the encouragement they need.

It is then up to these men to prove their

value, and it is up to the headmasters and school authorities to take advantage of the enthusiasm of such men, and to see that their energy and special accomplishments are put to the best possible advantage for the school as a whole. I believe these to be the best general principles but the details must vary with the school.

When I started at Marlborough in 1921 no provision of any kind was made for research for, as I indicated before, those in authority were definitely opposed to my doing it. In 1935, or fourteen years later, I was fortunate enough to obtain a Leverhulme Research Fellowship, and this happened to coincide with the building of a new set of laboratories on an extensive scale. The three men responsible for the sixth-form work in chemistry, physics and biology were each allotted a separate private room, and then so far as laboratory accommodation was concerned I was certainly well provided for.

Between the years of 1938 and 1949, that is 11 years, I went to teach at five other public schools all of which were more or less comparable with Marlborough. In four of them special accommodation had been set apart for the staff to do research, but in no case was any done.

Time for research

This raises another vexed question, but I feel that there can be only one answer. Schools are not research institutions, and every member of the staff therefore should be given his full share of teaching and supervision duties.

When a young man joins the staff of one of our public schools, he is expected to throw himself into the general activities of the school. He generally becomes attached to one of the Houses and thus gets his foot on to the first rung of the ladder leading towards a housemastership. Many men thus make a great contribution to the so-called teaching profession, but it is by no means the only type of contribution which can be made. Some men will make their best contribution by devoting their whole effort to their teaching. Such men are frequently penalised from a financial point of view, but they certainly get their reward if they are teaching in a really first rate school, *and* under a first rate headmaster.

At Marlborough I set apart a definite time for research, which was from 7.30 till 11.30 in the evenings, and of course during the vacations. It is frequently maintained that schoolmasters simply have no time to do research. Surely the lie is given to this statement by the fact that every schoolmaster gets at least three months' holiday during the year.

Equipment and the cost of apparatus used for research

This is a very difficult question and one on which it is unwise to dogmatise. If a man is going to pursue a branch of research, is he called upon to pay for his own apparatus and equipment? To this I can only reply again that schools are not research institutions. A man should certainly be provided with ordinary laboratory equipment, but if he needs special apparatus he must provide it himself or obtain it for himself. I owned the whole of my apparatus beyond ordinary glassware. I have always used the very best microscopes available and I can honestly say that no school has ever bought me a microscope, nor have I ever asked a school to do so.

Where is the money to come from? To this I believe there is only one answer. If a man is really determined to do a certain thing then lack of funds will never stop him. If he fails, then it will be from a lack of enthusiasm and not from a lack of funds. This is not really as bad as it sounds for there are such things as Government grants, issued through the Royal Society where reasonable requests receive reasonable consideration. Again there are such things as Leverhulme Research Fellowships and grants, and it is not impossible to obtain help in this way.

State-aided Secondary Schools

I have confined my remarks so far to public schools and originally intended to speak only of them. I have taught science in ten public schools, and only one State-aided Secondary School. There matters were certainly difficult. I had to teach biology to practically the whole school which contained about 400 boys, but there was no biological laboratory of any description. There was a good laboratory for chemistry and another for

physics and I had to use one of these when they were not otherwise required. Anything in the way of practical research was almost impossible, but this school was an ideal place for writing up previously collected data. My duties were such that I averaged well over two hours daily on my own work.

That a certain amount of research is done by school teachers is proved by the fact that a number of them work at the Plymouth Laboratory during their vacations and quite a number of these, if not the majority, come from State-aided Secondary Schools. Again, the *School Science Review* frequently publishes articles which definitely entail research on the part of the authors, many of whom are teachers in the State-aided schools.

The influence of research on teaching

In carrying out a piece of research, which was exacting but hardly unduly expensive or requiring special apparatus, I became a member of the Linnean and the Zoological Societies and of the British Association. I was by that time one of the recognised authorities on freshwater crustacea in the country and was brought into contact with university research workers.

Research of this kind is bound to have the effect of giving the research worker some self-confidence and a certain amount of self-respect. And it is not a bad thing to be able, when it is necessary to do so, to challenge some university pundits.

Now I put forward the following in all sincerity and I think it is reasonable.

When a man takes charge of some branch of sixth-form teaching a certain amount of success is expected. This brings with it the temptation to concentrate on the more brilliant boys. In other words, it leads to scholarship grubbing. Scholarship grubbing is not nearly so prevalent in our public schools as some of the professors at some of our universities would have us believe.

Be that as it may, real scholarship grubbing can do an enormous amount of harm. The heads of departments at the universities are fully justified in concentrating on the brilliant men but the schoolmaster is never justified in doing so.

A schoolmaster whether he is taking either the sixth form or the lowest form in the school, should be doing his very best to get the best out of every individual boy entrusted to him.

In most of the public schools the best boys tend to go on to the classical and mathematical sides. This means that, on the whole, the more brilliant boys are not likely to come on to the science side and my own experience was very definitely that you generally got very much the *leavings* taking biology. I think that this is a fact that most biologists must face. Keen as I am on biology, I yet disagree most thoroughly with the modern view that there is any special educational value attached to it.

It is a tremendous advantage for medical students to learn their elementary biology at school and pass their 1st M.B. but this is simply because elementary biology is so much better taught at many schools than it is at most universities. Naturally a good many men teaching science at our schools resent this state of affairs, but schools are provided primarily for the benefit of those being taught and not of those who teach.

I have met more than one man taking sixth-form biology who has admitted quite frankly that he considered it was his job to teach the scholarship men but he was not called upon to bother with the 1st M.B. people. Not only is this quite wrong, but experience has convinced me that by taking up this attitude a man runs a very grave risk of losing his very best pupils. Finally, the man who concentrates on the best pupils only must miss the late starter.

If one is faced with the problem of trying to help a heterogeneous crowd of boys, some of whom appear brilliant from the start, while others appear to be just the reverse, and if one is called upon to do the very best for each one of them, how is it to be done?

To my mind there is only one answer, namely 'You must be at your very best yourself,' and I am convinced that the only way you can do this is to set apart at least one hour daily for your own research. Throw everything else on one side for the time being. Realise for yourself, if you possibly can, your own limitations and I know of nothing which will better enable you to deal with the limitations of others.

MUSCULAR MOVEMENT

At the Birmingham meeting of the Association the Physiology Section held a session, under the Chairmanship of Prof. R. J. Brocklehurst, in which various aspects of muscular movement were discussed.

The topic was introduced by Mr. W. Gissane, of the Birmingham Accident Hospital and of the Department of Surgery, Birmingham University. Mr. Gissane first expressed a conviction that if the surgery of the locomotor system was to continue to advance, particularly in the repair of injury, first there must be a more complete understanding of the biological and mechanical principles governing muscle movement, followed by teaching of these principles to surgeons.

He felt that the surgery of repair, following injury at least, should be capable now of advancement from an art to a truly exact science if a serious attempt were made to co-ordinate knowledge already gained by anatomists, physiologists and orthopaedic surgeons. It was perhaps a fair comment to say that much of operative surgery is based on the craftsmanship of operative technique—demanding a sound knowledge of descriptive anatomy but not based upon a sound knowledge of the anatomy of function or muscle movements.

Many operations, he recalled, had been fashionable for a time and were then discarded, having failed to achieve their purpose—a system of advancement by a trial and error technique. Unfortunately, in this race to discover new operative techniques, fundamental knowledge in our understanding of functional anatomy was uncovered but quickly forgotten. For example, there had been a great deal of confusion concerning the treatment of recurrent dislocation of the shoulder, the traumatic anatomy of which was very accurately described by Broca and Hartman sixty years ago. Mr. Gissane described the fundamental lesion involved, i.e the detachment of the glenoid labrum and capsule with a stripping of the periosteum from the front of the neck of the scapula and a traumatic defect of the head of the humerus; the head subluxated anteriorly over the glenoid margin whilst still remaining within the detached capsule when the arm was abducted and externally rotated. After many different types of operation had been recommended, there was now, at least in this country, a measure of agreement in the treatment of the condition, with due regard to the nature of the essential lesion.

Mr. Gissane then reviewed some everyday problems that confront the practising surgeon in his efforts to restore normal movements to the locomotor system following injury. There were very few structures which did not participate in normal muscular movements: the skin, the superficial and deep fascia, the muscles themselves, the named ligaments and capsules of joints, the joints, the bones, and the blood and nerve supply to each structure. The function of the skin and its mobility upon its underlying fascia was perhaps best illustrated in the hand. In this region even clean-cut surgical incisions had to be confined to areas where the skin has little or no mobility on its underlying superficial fascia. This was illustrated by slides showing the correct and incorrect types of incision in the hand. Those which cross creases and mobile fascial planes at right angles could anchor skin and its underlying fascia and cause scar contractions and loss of normal range of finger and hand movement. Such incisions were unfortunately still prevalent. In contrast, the well planned and correct skin incisions in the hand, affording access to any hand structure, did not in themselves cause a loss of movement.

Mr. Gissane then referred to the function of the skin in movement, as may be seen after badly treated burns and scalds. Slides were shown to illustrate the effects of burns of the skin of the hand, sustained forty-five years ago, and treated according to the practice of those days. The results

were characterised by deformities due to shortage of skin. These deformities were in part due to fibrosis and scarring caused by infection. It was now possible, as a result of the research of clinical bacteriologists, together with the practical application of that research, to use techniques of wound dressing whereby infection in most open wounds was eliminated. Skin grafting as early as possible was now employed, and slides were shown to illustrate the remarkable successes now commonly achieved by suitable technique in the treatment of extensive burns involving the whole skin thickness.

Mr. Gissane said that, in general, if skin loss were made good by aseptic surgical methods within three weeks of its occurrence, return of full movement was quickly achieved without the aid of physiotherapy. If this healing period were prolonged, return of the full range of movements was slow, and in many instances might never be complete in spite of final adequate skin cover, the elimination of infection and intensive rehabilitation.

It seemed, at present, that the primary cause of this loss of movement was the thickening of exposed fascial planes, not only because they were exposed, but because they have been rendered immobile by pain and the inability of the patient, for many reasons, to move the damaged extremities. In the experience of the speaker, provided secondary shortening of muscles, ligaments and capsules, with permanent changes in joints had not yet occurred, full correction of deformities could be obtained by exposure and excision of thickened fascial planes.

Fascia was accepted as the sliding medium not only under the skin, but, in the case of the deeper fascia, between various muscle planes. In order to retain its normal function of a sliding medium it seemed that the fascia must continue to be acted upon by sliding movements of the muscles or tendons. An instance of a greatly thickened tendon sheath in the palm of the hand was quoted, where the underlying cause seemed to be the closed division of the flexor profundus tendon of a finger. Infection in this case could be excluded, the thickening apparently originating in the loss of movement of the tendon. The main problem was how to combine the period of rest essential to the repair of a divided structure, with the mobility necessary for the return of full muscular movements. In the past great emphasis had been put on rest for prolonged periods, but now the emphasis was on rehabilitation by early muscle movements. The latter aim was limited by the need to respect the fundamental surgical principle of rest of a divided structure to allow of its repair, and in fact the two desirable objectives seemed to run counter to one another. This was illustrated by the case of prolonged immobilisation following fracture of the femur, which would produce good union and alignment of the bone, but at the same time the mobility of the knee, despite rehabilitation, might never again reach its original range. Even methods of treatment of such a fracture by allowing knee movement, did not always give the desired range.

Mr. Gissane thought that the stiffness of the knee-joint was more logically attributable to damage to the quadriceps muscle group. He described one method of treatment in which the fracture is treated by immobilisation of the knee on a Thomas splint, but during this period the quadriceps group are exercised by static contractions followed by relaxation over given intervals during the day, so as to retain their gliding movements over a small range. In this treatment intractable knee stiffness does not occur, unless the intrinsic damage to the quadriceps has been great at the time of injury.

Recently, in the treatment of peripheral nerve injury, surgeons had been impressed with the value of ' lively splints ' during the recovery phase of paralysed muscles. It might be that these splints allowing, as they do, of passive movement of paralysed muscle groups, were able to retain the required activity of the various fascial planes, in addition to their other functions.

The relation between muscle movements and the function of ligaments was next referred to by Mr. Gissane. He preferred to regard ligaments as actively participating in the pattern of movement of the limb as a whole, and instanced the importance of the integrity of the whole of the ligamentous structures of the ankle and subtaloid joint in normal locomotion. He described studies on the movements of

bones in normal locomotion, made at the Anatomy School of the University of California. These studies were directed to the acquisition of fundamental knowledge required for improvements in the design of artificial limbs. In this country, limb fitters and makers of artificial limbs had to a large extent dictated the level of lower limb amputation. The limbs used here were made with a simple controlled hinge joint at knee and ankle. Surgeons had not been particularly happy about this trend but so far had lacked the necessary fundamental knowledge to challenge it. The Californian research had now indicated that transverse rotations between the various segments of the lower extremity are an important factor in the ease and rhythm of walking of the normal individual. The relative movements of pelvis to femur and femur to tibia would seem to indicate an intimate and co-ordinated function between muscles and joint ligaments.

Mr. Gissane finally expressed the opinion that in orthopaedic work the help of the clinical anatomist was needed ; he would help the surgeon by assessing the clinical application of knowledge gained in the fields of pure science, would prosecute further research and sift the wealth of knowledge gained in the more empirical methods of the past. In this way logical advancement could be secured.

Mr. Gissane's introduction was followed by three papers read by Dr. H. D. Darcus, M.R.C. Unit for Research on Climate and Working Efficiency, by W. F. Floyd and P. H. S. Silver, Departments of Physiology and Anatomy, Middlesex Hospital Medical School, and by Dr. L. Guttman, Ministry of Pensions Hospital, Stoke Mandeville.

Development of Isometric Contraction

by

Dr. H. D. Darcus

In the present discussion, an attempt will be made to bring together some of the knowledge and problems concerned with the development and determination of one form of muscular activity—isometric contraction. When a skeletal muscle contracts, the energy of contraction is either used in producing movement or, if the movement is prevented, in developing tension in the muscle. When the tension of a muscle is maintained without causing a movement, we say that the contraction is isometric ; that is, the contraction results in no alteration in the length of the muscle as a whole. This does not imply that either the tension or the length of the individual constituent muscle fibre remains constant.

The tension of the muscle fibres undoubtedly fluctuates in response to the arrival of successive volleys of impulses from the central nervous system along the nerves to the muscles. These volleys reach the different groups of muscle fibres in the same muscle at different times, so that certain muscle fibres may be developing tension while others are relaxing. In this way, the over-all activity of the muscle is kept uniform.

The fluctuations in tension of the muscle fibres may be accompanied by alterations in their length, shortening as they develop tension and returning to their initial length when the tension is relaxed. The length of the muscle as a whole does not have to change, as the shortening of certain muscle fibres can be compensated for by the stretching of others.

From time to time, investigations have been made on the force of isometric contractions in man, but advances in this field have been sporadic. Apart from its intrinsic importance in the problem of muscle mechanics, clinicians have been interested in the determination of this force as an integral part of physical diagnosis, as a guide to treatment and as an index for assessing the effects of treatment. Engineers have sometimes directed their attention to these problems because of the need for this knowledge in the design of machines both for peace and war. Other groups of investigators have been prompted to conduct experiments on the measurement of muscle strength in an effort to find some criteria of normal physical efficiency.

Methods of Determination

In spite of this wide range of interest, there is still no commonly accepted method of measuring the tension developed during isometric contraction.

In general, clinicians still rely on a subjective assessment of muscle strength which depends on using themselves as the test instrument. This, although not without its use as a rough test, is unsatisfactory because it does not provide an accurate or a reproducible objective measure.

There have been numerous attempts to design a standard objective test procedure, this, in the majority of cases, depending on the degree of stretching or compressing springs. However, one of the main objections to an apparatus using springs is that it does not permit a wide range of muscle force to be determined with accuracy.

In an effort to overcome this objection, a

dynamometer has now been designed in which the muscle force is allowed to deform a tempered steel bar, and the degree of deformation recorded by electrical strain gauges. If the bar is of sufficient strength, the movement allowed on contraction is almost negligible even with maximum muscular effort, so that as nearly as is practicable an isometric contraction results. As the sensitivity of this apparatus can be readily varied within wide limits, it can be used to measure a large range of muscle force.

Functional Organisation of Muscle

Impulses from the central nervous system are conducted to the muscle by a large number of nerve fibres, each of which divides and activates a certain group of muscle fibres. This group is called the motor unit which is the smallest functional sub-division of the muscle. The number of fibres per unit varies within the same muscle and also from muscle to muscle. There may be as few as six or as many as two hundred.

There is reason to believe that the size of the motor units bears some relationship to the function of the muscle. Thus the motor units are smallest in those muscles in which it is necessary to produce finely differentiated tension or movement, such as those which are responsible for the precise actions of the fingers and the thumb, for movements of the eye-ball, for adjustments of the vocal cords and for alterations of facial expression ; whereas they are larger in the powerful muscles of the limbs which are required for coarser adjustments. In the same muscle, there is probably a relationship between the size of the motor unit and the tension developed, smaller units contracting when small tensions are exerted and larger units coming into action with greater tensions.

Development of Isometric Force

The degree of isometric contraction can be delicately graded to suit the task to be performed. These gradations do not depend on each single muscle fibre contracting to a varying extent, for, under normal circumstances, once a muscle fibre is stimulated to contract, it does so maximally. It depends on the proportionate number of motor units contracting and the rate of their activity ; the more motor units in activity and the greater the frequency of their discharge, the higher the tension.

When a contraction starts in a muscle, initially there are very few motor units active, perhaps only one. The number of motor units contracting and the frequency of their contraction increases progressively as the tension is increased. At any particular tension, the same motor units are rhythmically contracting and they continue to do so as long as that tension is maintained.

As the isometric force is increased not only is the tension within any one muscle gradually raised by the recruitment of motor units and an increase in their frequencies of contraction but also more muscles are called into action. For instance, in supination of the hand when only a small resistance has to be overcome of the muscles concerned in this movement—the supinator muscle and biceps—only the supinator contracts, biceps assisting when the resistance exceeds a certain limit.

When a maximum isometric effort is made, there is a time lag before the tension in the muscle reaches its maximum value. There are two factors which may be responsible for this delay. Firstly, this period may be necessary to allow the maximum number of motor units that can be excited to commence their rhythmical contractions and to reach their maximum rate of activity. Secondly, during this phase, part of the tension of the contracting motor units may be expended on the lengthening of muscle fibres of inactive motor units or on the stretching of other elastic elements within the muscle.

Thus, we may picture the initial increase in the tension of the muscle as being due to more motor units becoming stimulated and to less tension being absorbed by the part passively stretched.

The effect of joint position on maximum isometric contraction

It is a common experience that one can exert a greater maximum isometric force in a certain position of a joint than in others and, under normal circumstances, one instinctively assumes this position when a large resistance has to be overcome.

The effect of joint position varies in different joints. For instance, in flexion of the elbow, the maximum isometric force is developed towards the middle of the full range of movement. In pronation of the hand, the maximum is achieved when the hand is fully supinated and the force decreases linearly as the hand is pronated ; the reverse is true of supination.

(*a*) Mechanical advantage.

The applied muscle force which is measured in the contraction of a muscle is the resultant of the tension of the muscle and its mechanical advantage. As the angular position of the joint is changed, the direction of the pull of the muscles acting on it is altered, and thus the details of their lever mechanism is varied. There is always one phase of the movement in which the muscle is working at its greatest mechanical advantage. This point is not necessarily the same for each of the muscles co-operating in the same movement.

It is often said that the effect of leverage of

the muscle outweighs all the other factors and is, therefore, presumably responsible for the form of the tension-joint position curve. In certain instances this certainly appears to be the case. For example, in flexion of the elbow from full extension, as previously mentioned the force exerted rises until about the middle of the range of movement and then falls with further flexion. Similarly, the lever arm of the muscles producing flexion increases until the elbow is bent to nearly a right angle and then decreases.

However, changes in mechanical advantage, although obviously important, may not be the only or even the most important factor affected by changes in joint position. This is indicated by the fact that in certain joints the forces exerted do not follow the changes in leverage of the muscles.

(*b*) The contractile force of the muscle.

One factor which probably plays a part in variation of muscular tension in different joint positions is the degree to which the muscle is contracting. There is some evidence which suggests that the number of motor units active in different positions of the joint is not constant. There are certain positions in which more motor units can be caused to contract than in others, thus resulting in alteration in the tension of the muscle.

Furthermore, as muscles do not act alone but in groups, it is necessary to consider the number of muscles active and the relative degree of their activity when tension is developed or movement produced. Each muscle of the group has its own sphere of action, which may not be coincident with that of associated muscles, so that the applied muscular force of the various muscles is not necessarily maximal for each in the same position of the joint. In extreme cases a muscle may not be effective over the whole of the range of movement in which it plays a part so that there may be different combinations of muscles acting in different joint positions. For instance, in supination of the hand, biceps ceases to contract towards full supination and it is left to the supinator muscle to carry on. The contribution that each muscle makes to the applied muscle force in different positions of the joint will depend on its mechanical advantage and the degree to which it can be caused to contract. In the case of some muscles, there may be positions in which, even if they do contract, they can produce no tension because the distances between the points of their attachments are less than the length of the fully shortened muscle. Here again we can instance biceps which, when caused to contract fully by direct stimulation, is unable to bring the hand into full supination.

The relation between the length of the contracting muscle and its resting length has also been proposed as a factor affecting the contractile power of a muscle. It has been shown in single muscle fibres and in isolated whole muscles that as a muscle lengthens, so its force increases, until it reaches its resting length, after which further extension causes a falling off in the contractile force.

There are experiments which suggest that the same phenomenon occurs in the intact body. In plantar flexion of the foot, the force of isometric contraction decreases as the foot is plantar flexed and as the knee is bent. Both these procedures result in a shortening of the muscles acting in plantar flexion without any marked change in their mechanical advantage.

Various explanations have been advanced for the changes in contractile power with alterations in the length of the muscle: (i) Extending and shortening of the muscle may affect the contractile properties of the muscle fibre. (ii) It may also alter the architecture of the muscle. In the majority of the muscles of the body, the constituent fibres do not run parallel to the long axis of the muscle, but are oblique to it. In such a case the tension they can exert in the long axis of the muscle will be correspondingly less. As the muscle shortens, the obliquity of the fibres increases and, therefore, the resultant force is further reduced. (iii) It has also been postulated that the connective tissue framework of a muscle may play some part in reducing the effectiveness of the contraction as it shortens. (iv) Apart from these mechanical aspects, it is possible that the change in tension which occurs with alteration in the length of a muscle may be a reflex effect. When a muscle is stretched, certain specialised receptors—the muscle spindles—are stimulated. Impulses which arise from these receptors can reflexly cause the muscle to contract. The more the muscle is stretched the more receptors that will be stimulated.

Daily variations in maximum isometric contraction

Apart from the variations found in isometric force in different positions of the joint, wide variations are found in the force exerted on successive occasions with the joint in the same position. It is probable that some of these variations may be accounted for by the inability to reproduce exactly the position of the joint, however carefully the experiments are controlled.

However, some variation may be due to alterations in the response to maximum voluntary effort. It might be expected that there would be an improvement in performing the task with practice. This could result from an increase in the contractile force of the muscle fibres or to an increase in the degree of excitation of the muscle or in the number of muscles

called into play which might be attributed to the muscle responding to less intense nervous stimuli or to the subject being able to exert a greater voluntary effort. However, it has been found that in isometric contraction the 'training' effect is not marked and takes a considerable time to show itself. This may indicate not that the improvement in the neuromuscular mechanism is slight but that it has been masked by other variable factors, such as muscular fatigue.

Finally, probably the most potent cause of fluctuations in the tension developed in response to maximum voluntary effort, is the inconstancy of the will of the subject to exert himself to the utmost.

Conclusion

It is hoped that this brief survey has given some picture of the problems involved in the study of isometric contraction in the intact human body. Although advances have been made in this field, there are still many aspects that require investigation, but before this is possible, standardised methods of measuring muscle force will have to be adopted and methods devised of determining accurately the factors involved.

Electromyographic Studies of Patterns of Muscle Activity in Posture and Movement

by

W. F. Floyd and P. H. S. Silver

Muscle action potentials were recorded with a six-channel amplifier and ink-writing oscillographs, surface electrodes being placed on the skin over the various muscles as previously described by us.[1] The patterns of activity in six different muscles, recorded simultaneously, have been correlated with the position of the centre of gravity of the body relative to the points of body support.

In the upright position, when the subject is standing comfortably and at rest, the amount of activity found in trunk muscles and muscles of the upper leg depends on the position of the centre of gravity. There is always a body position which gives minimal (practically zero) muscle activity. When the centre of gravity of the body is displaced by a small amount, reflex contractions of these muscles are evoked which tend to return the centre of gravity to the original position. When a new posture is adopted in which, however, the centre of gravity remains unchanged, the pattern of trunk and leg muscle activity also remains practically unchanged, except for the activity of those muscles concerned primarily with the maintenance of the new posture. Thus, if a weight is carried in the left hand without any lateral displacement of the trunk from the position of balance without the weight, the contralateral external oblique muscle contracts (and in some subjects the contralateral erectores spinae as well). If an equal weight is placed in each hand, however, these muscles show no activity other than that which was present previously, i.e. before the weights were carried. In the first instance, when one weight is carried, the centre of gravity of the body plus load is displaced laterally and the contralateral muscular contraction counteracts this displacement. In the second instance there is no change in the position of the centre of gravity, and hence no compensatory muscular action is required.

When the subject bends forwards, the lumbar erectores spinae muscles contract to counteract the forward displacement of the centre of gravity of the trunk. Other muscles such as the glutei also contract but are outside the present discussion. When the trunk is bent sufficiently far forwards, as when the subject attempts to touch his toes, it is found that the erectores spinae contract more and more vigorously until the lumbar vertebrae are fully flexed. At this point they relax suddenly, as though an inhibitory mechanism were brought into play. This relaxation continues while the trunk is bent further forwards and the movement of touching the toes is completed. On reversal of the movement the erectores spinae do not contract until the lumbar vertebrae are just beginning to be extended: contraction then starts vigorously and diminishes until the upright position is reached. The same pattern of activity is found when the subject bends down to lift up a heavy weight.

The strain due to the weight of the trunk and the load, under these conditions, is carried by the ligaments and other non-contractile structures when the erectores spinae muscles are relaxed. In particular the strain imposed on the intervertebral discs is increased, and it is clear from anatomical and mechanical considerations that rupture of the annulus fibrosus (the lesion of the 'slipped' disc) is most liable to occur during this period when the erectores spinae are relaxed and all the strain is thrown on the non-muscular structures.

Study of the erectores spinae muscles when the subject is seated shows that the position of full flexion of the lumbar vertebrae is likewise one of inactivity of the erectores spinae. When the lumbar vertebrae are only partially flexed, however, the erectores spinae contract even though the subject may be leaning against the back of a chair, and obtaining support from it. This slumped position is inelegant but is nevertheless the position of minimal or zero trunk muscle action.

[1] Floyd, W. F. and Silver, P. H. S., 1950, *J. Anat.*, **84**, 132.

Restoration of Movement in Paralysed Muscle

by

Dr. L. Guttman

In this communication the problem of restoring function of paralysed muscles by compensatory action of muscles in normal parts of the body is discussed. This has been known for a long time in paralysis of muscle groups of extremities, where the lost function can be effectively compensated for by what is generally called 'trick movements' of normal parts of the limbs. To what extent such trick movements can be trained and developed is shown in a patient who, after diving into shallow water, broke his neck and sustained a complete lesion of his spinal cord at the level of the sixth cervical segment. He was left with the use of two muscles of the hand, namely the extensor carpi radialis longus and flexor carpi radialis, while all flexors of the fingers and thumbs were completely paralysed. This patient gradually learned to flex the fingers passively, by extreme extension of the wrist, to such a degree that he became capable of holding objects and even managed to shave himself.

However, little has been known about utilisation and compensatory training of muscle of the trunk to overcome extensive paralysis of large parts of the body, including both lower limbs, which always occurs after complete lesions of the spinal cord, whether caused by injury or inflammatory disease, such as transverse myelitis or poliomyelitis. In the past, a man with a complete lesion of the spinal cord at the level of the mid or upper thoracic region, let alone those with lesions of the cervical cord, was considered as a helpless cripple. As a rule, he was confined to bed for life or had to be transported in lying position in a spinal carriage. If the upright position in such cases was attempted, it was achieved by propping them up with artificial aids, such as heavy leather or steel corsets. And yet nature has provided us with certain facilities to discard these artificial aids, by utilising and mobilising certain muscle groups of the trunk, which can guarantee the upright position of the paralysed person and, in certain circumstances, even restore, to a certain extent, his capability to walk again. These are those muscle groups which have at least part of their attachment to the pelvis, i.e. the front, the abdominal muscles and, at the back, both the middle and lower portions of the trapezius and, in particular, the latissimus dorsi. The latissimus dorsi and the two lower thirds of the trapezius, if made strong enough by training, can adequately control the stability of the hips, even if the normal hip-stabilising muscles are paralysed.

The latissimus dorsi has its insertion in the upper end of the humerus and its origin from all the spinous processes from the sixth thoracic vertebra downwards, the sacrum, and most important of all from the posterior rim of the ilium. The segmental innervation of this muscle is as high as cervical 6, 7 and 8. Therefore here is a muscle which, in any complete lesion of the spinal cord which is compatible with useful life—i.e. in complete transections of the spinal cord at any thoracic level up to thoracic 1, and even in the most distal part of the cervical cord—by-passes the spinal lesion and thus bridges the remaining normal part of the trunk with its paralysed portion. In normal circumstances, the latissimus dorsi will adduct, retrovert and internally rotate the humerus. If, however, the arm and the shoulders are fixed, the mobilising end of this muscle is transferred to its origin, and a pull is exerted on the pelvis and results in tilting the pelvis in an upwards direction, and, consequently, by fascial stretching produces extension of the hips. The upwards pull of the latissimus dorsi is supported by the same action of the lower third of the trapezius, if the shoulders and scapula are fixed, as this muscle too can apply an upwards pull from its origin at the region of the twelfth thoracic vertebra—in other words, from the upper end of the lumbar curve—thus straightening the lumbar lordosis to a certain degree. It is obvious that the lower the lesion of the spinal cord the more the number of muscles of the trunk that can co-operate, and in all lesions at the level of the distal portion of the cord, the abdominal muscles, especially the obliquus externus, participate in the team-work of pelvic tilting muscles, thus restoring the walking capabilities of the paralysed if the knees are fixed by simple appliances. Amongst the methods which have proved invaluable in restoring the physical and mental rehabilitation of paraplegics, sport has played a most essential part, and the writer has proved that many sports can be adjusted to the limited facilities of paralysed persons.

SOME ASPECTS OF THE STUDY OF SETTLEMENT IN BRITAIN[1]

by

EMRYS JONES

Of all the distributions which the geographer studies the distribution of man over the earth's surface is probably the most important—and surely the most interesting in view of man's irritating habit of turning up in the most improbable places. But humans are exceedingly mobile, and we can do no better than study symbols of relative permanence, dwellings and other structures, and ask ourselves why certain locations were chosen in preference to others. As requirements, technical advances and even trends in fashion change so rapidly, and as these cultural factors are so integral a part of the problem, it is as well to recognise at the outset that the contributory factors in any given distribution must be considered in relation to the period in which that was determined.

The physical background across which this human movement takes place is the surface of the earth, its hills and plains; its drainage and vegetation; its climate and the initial richness or poverty of the soil. All have a bearing on man's distribution.

It is well-nigh impossible to discuss settlement without revealing which of these sets of factors, cultural or physical, is thought to be more important. The difficulties of committing oneself unreservedly to one side, and dismissing the other, are enormous. Much as the geographer would like to find that kind of correlation which would lead to a 'new determinism,' it sometimes appears that man's seeming irrationality will always defy such attempts. On the other hand, man has found it to his advantage to take cognisance of his physical setting, and therein lies the close inter-relationship which we seek to define. Given, therefore, any region of settlement in a country such as Great Britain, it is very tempting to end one's enquiries indefinitely on this issue and say, 'There is much to be said on both sides.'

Most writers make plain which factors they consider salient; and their conclusions seem very often to depend on *a priori* considerations. Proceeding from the physical side one can explain much in terms of natural features, dismissing the exceptions as due to 'human factors'—a phase which covers a multitude of awkward misfits. From the human side there is the danger of abstracting the settlement, in the form of dot maps, and studying it *in vacuo*, thus negating the physical environment which may hold part of the answer. In either case we are shown only fragments of the problem.

The number of studies in rural settlement in Britain is not very great, but even so it would be difficult to describe, let alone classify, the studies which have appeared since a greater impetus was given to this work in the International Congress of 1925. They certainly illustrate the width of the problem and the number of divergent aspects which it includes. Professor Bowen's early correlations of settlement patterns and ethnic groups (1), and Dr. Garnett's studies of the effect of insolation (2) may illustrate the extremes. Others have sought explanations in land features, geology, soil, economy, land tenure and cultural features.

Where the entire range of causal factors is sought, rather than the effect of a single factor, many of the studies end with as conspicuous a question mark as that with which they started. For example, some who attempt a physical correlation to explain settlement distribution conclude with a hint that tenure may hold a vital clue still to be found.

[1] Paper read to Section E (Geography) at Birmingham on September 5, 1950.

Those who examine cultural factors protect their flanks against the formidable attacks of physical geographers by adding a rider concerning the probable influence of topography and soil. One set of factors, physical or human, is an insufficient key to the entire problem of settlement distribution. One may have to agree with Fawcett (3) that the broad limits are physical and that the more detailed distribution is determined culturally; and further that only in marginal cases of poverty of water or cultivable land are purely physical factors decisive.

Miss Sylvester, in a recent study on Anglesey (4), tried to show the operation of both sets of factors on the distribution of settlement in that island. On the evidence given, however, these factors seem at variance when applied to distribution considered as a single issue. It is significant, for example, that soil differences, although they affect density, never really affect pattern of settlement in Anglesey.

Although the problem has often been studied as though the issue were a simple one of distribution, this is not the case; it is a complex problem with several aspects. Does 'distribution' mean the number of dwellings in certain units of land? Does it mean the extent of settlement within certain limits? Or does it mean degrees of dispersion and agglomeration? Explaining the presence or absence of settlement in an area is in quite a different category from explaining why it should be dispersed and not nucleated. Clarifying these questions may make it easier to distinguish which set of factors is operating in a specified case.

A warning which must first be heeded is this: analysing settlement study into these components for greater ease in handling data must, to some extent, break down what is essentially a single landscape feature in which these many strands are inextricably woven. Even while the investigator is isolating a part, he must be conscious of the whole. Analysis of this kind is merely a tool by means of which it may be easier to recognise the different causal relationships whose sum total gives rise to the human landscape.

There are at least three aspects and three sets of relationships which can be distinguished:—

i. There is the relationship between the single dwelling and the surface of the land. In a scattered habitat each dwelling could have claims to be studied separately, but more often the advantages of certain sites are shared, sometimes giving rise to a village. Purely physical though this aspect may seem to be, the utilisation of sites may warrant an investigation of the society. Defensive sites reflect the need for defence as surely as they reflect the use of land forms as a means of attaining it.

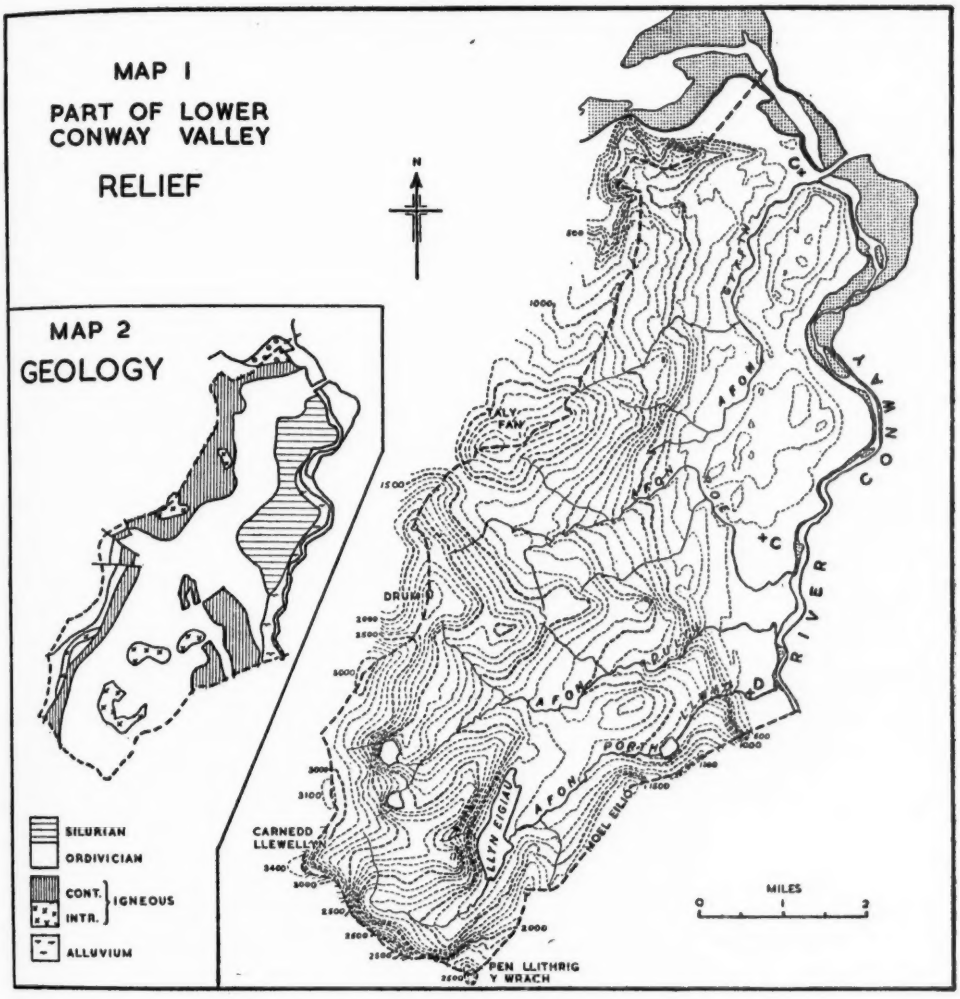
ii. There is the relationship between one dwelling and other dwellings in the same group. This gives the pattern of settlement. Here we are dealing with nucleated and dispersed settlement, or with degrees of dispersion.

iii. In a much wider context, one would have to consider the extent of an area of settlement and its limits. By this is meant, for example, the utilisation of a valley side between the flood plain and an upper altitudinal limit.

These aspects—site, pattern and limits—present separate problems, and the solving of one does not necessarily imply the solving of the others. To refer to the simple example mentioned above; although a series of sites may be satisfactorily explained in terms of physical factors they may not affect the pattern at all, for it is the choosing of actual sites from a greater number of potential sites which gives the pattern.

It may well be that many of the differences between the physical and human schools in this matter could be resolved by a recognition of these different sets of relationships. The problem of siting is the task of microphysiography: patterns must be analysed via cultural and historical studies: limits of distribution may be physical when they are absolute—like the absence of water—but in Britain are more likely to be a balance between physical and cultural, reflecting degrees of human adaptation to the physical environment.

Seeking causal relations in the settlement of Britain is no easy matter, and nowhere are its complexities more apparent than in Wales, where the intricacies of the former cultural background allow only fleeting glimpses of many contributing causes. Recent studies have, however, afforded a much clearer view of the problem, and in these may be seen the



Relief and Geology of part of the Lower Conway Valley

possibilities and the value of the analysis suggested above.

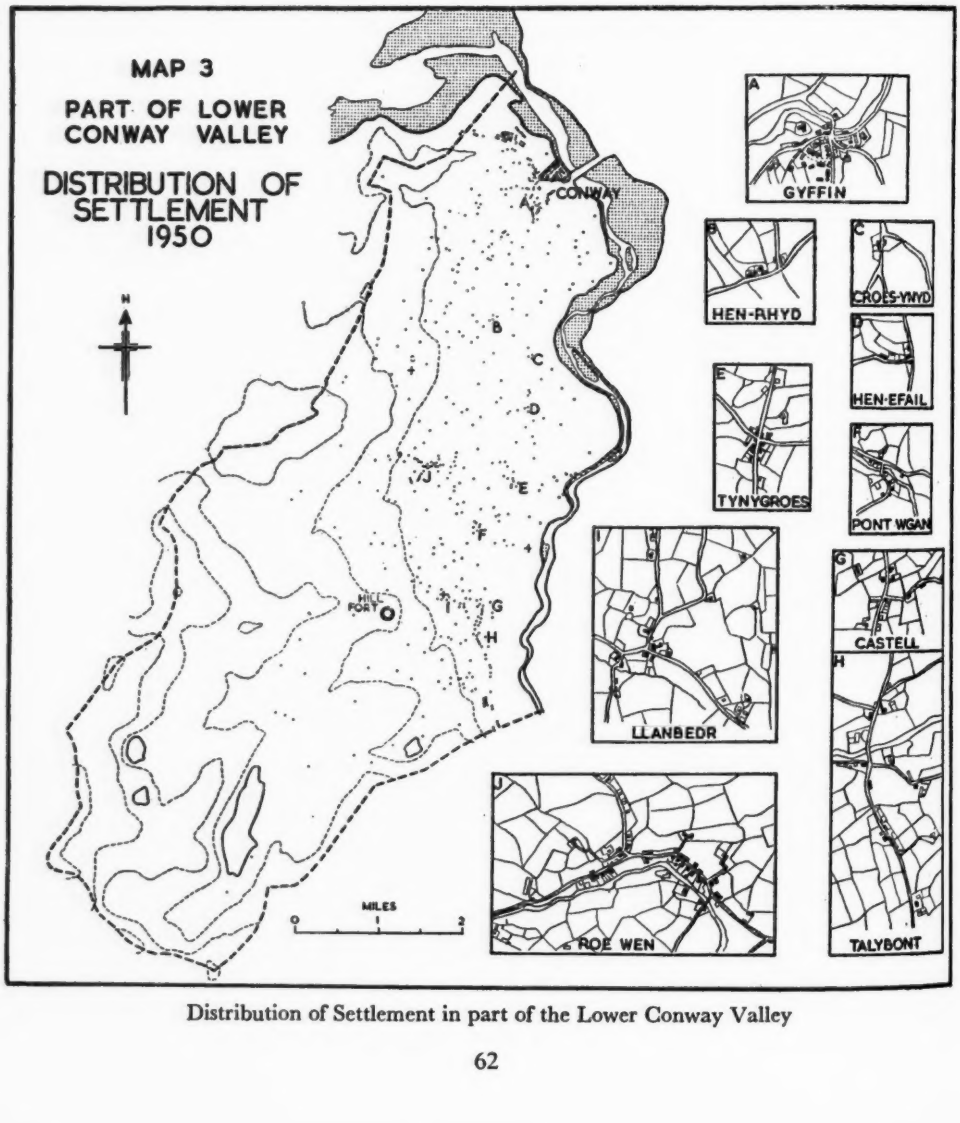
An area which has been studied most completely is that of part of the Conway Valley in North Wales, published material being available in articles by Professor T. Jones-Pierce ('The *Gafael*, in Bangor MS., 1939,' *Cymmrodorion Society Transactions*, 1942) and Dr. R. E. Hughes ('Environment and Human Settlement in the Commote of Arllechwedd Isaf,' *Transactions of the Caernarvonshire Historical Society*, 1940). It is significant that what must be recognised as major contributions in this field of study come from an historian and an ecologist. Such detailed specialised studies can only be gratefully accepted by the geographer to further interpret the landscape, though it reveals the need for combined working in the field.

The area subject to this detailed examination lies partly on the eastern facing slopes of Snowdonia, its limits being the water parting between tributaries draining to the Conway and rivers draining to the sea and to the Menai Straits, as far as Carnedd Llewellyn (Map 1). Many features of the topography are directly related to the geology of the area (Map 2), which is largely built up of Ordivician grits and shales giving fairly gentle and even slopes. Much steeper slopes are associated with igneous rocks, especially the group of contemporaneous felsites which give a

steep valley wall in the south, and give a steep edge on the rim of the valley further north. The two rounded eminences of Silurian shales are structurally part of the Denbigh mass to the east. There is considerable alluvium along the river itself.

The present-day settlement (Map 3) is so evenly spread—within the limits of altitude and the restriction of steep slopes (5)—that it can be called a fully dispersed pattern with some secondary modifications. The few clusters of houses which interrupt the scattered habitat fall into two classes, the road village and the tiny hamlet, often around a mill. Both are service centres, usually with an inn, a post office, a store and a school; the former smithy is now a garage. These centres hardly disturb the underlying pattern upon which they are superimposed, the *treflan* (6), in which the church gives unity to minor neighbourhoods which have no other outward expression of their social cohesion. The pattern of 1840, as shown in the O.S. First Edition, is identical in most respects, with the exception of the road centres referred to above which are products of the last century. In both cases the contrast is very evident between this dispersed pattern and that sign of limited intrusion, the walled town of Conway.

To Professor Jones-Pierce we are indebted for proving the derivation of this pattern. In an ingenious analysis of a



Distribution of Settlement in part of the Lower Conway Valley

rental record (Bangor MS., 1939) he has revealed a picture of this area in 1453, in the last stages of the decay of tribal institutions. This tract of country was a commote within which, among others, was the free township of Castell. This was probably formerly a single tribal territory inhabited by freemen, and may have been once an area of undifferentiated pastoral settlement, later having its centre in what is now the hamlet of Castell, where an original homestead (*hendref*) was situated. Around the *hendref* in medieval times were individual holdings of arable and pasture, parcelled according to *gafaelion*, family holdings within the tribal territory. Professor Jones-Pierce distinguishes 22 families, or clans, in Castell, and 22 *gafaelion*. The *gafael* was the united holding of the clan, a dispersed unit of land tenure showing great morcellation within for the purpose of *per capita* inheritance (8). When morcellation became extreme the *gafael* extended into the common pasture outside, which was settled when population pressure dictated.

Portions of the *gafael* would, then, be scattered all over the *tref* of Castell, in patches of arable and pasture. Of primary importance is the fact that on the death of the head of the clan the inheritance was divided, and the first distribution was that of new *tyddynnod* (homesteads) of a few acres, which were interspersed with the arable and meadow. The new sites would be limited to the scattered parcels of a particular *gafael*, or to part of the common if expansion was taking place. Professor Jones-Pierce, by identifying contiguous parcels, has been able to reveal the resulting patchwork, and within it are the *tyddynnod*, named in the rental and already dispersed. No group is in a block. Each *gafael* is a mass of small parcels with homesteads intermingled with the rest. This is the end product of a process of subdivision, accompanied by a strong sense of personal ownership. This last point cannot be over-emphasised. There was communal ploughing—how else could the scattered parcels be economically managed ?—but it was the sense of individual ownership which lay behind the isolated *tyddyn*.

The map of holdings in the mid-fifteenth century which Dr. Elfyn Hughes has compiled shows the complete dispersion which had occurred by this time. If one would want to speculate further into origins, the link seems to be the hill fort which dominates the whole *tref*. Whether this was a single tribal unit (10), and whether it represents in that case a former nucleated group which later moved to the valley before dispersion and expansion remains to be investigated.

The pattern, then, is the result of cultural evolution—cultural rather than social because Conway is there as a reminder of another, intrusive culture. This native pattern may have arisen from the devolution, due to the laws of tenure and inheritance, of a former possibly more compact form, maybe even a tribal house. Professor Estyn Evans has shown devolution of this kind to have occurred in nineteenth-century Ireland (11) : in Wales it was complete by late medieval times.

Much could be speculated on the limits of distribution from Dr. Hughes' studies with their invaluable contribution on soil and ecology (12), often an expression of structure, slope and rainfall, and former plant communities. On the thin upland soil is found moorland, mainly mountain fescue, with considerable areas of peat in the wetter south. The lowland brown soils, now arable, formerly carried oak woodland with very little of the original forest, there being a weaker growth on the Silurian hills. On the Silurian, forest gave way to pasture. The gley soils along the valley bottoms and on the deeper drifts of the gentler slopes still carry alder, ash, elm and willow, but much less than formerly. The sides of the valley show a variety of vegetation, but there was undoubtedly an oak-birch community on the truncated soils between 500 and 1,000 ft., to-day an area of upland scrub with much bracken and gorse. The patches of Molinia, still higher, may have been alder formerly.

Dr. Hughes has shown (13) that in the fifteenth century settlement was more concentrated on the lowland, on land which must have been extensively cleared of its oak woodland, although it may have been settled earlier than such a clearing implies. Then there was considerable expansion outwards at the expense of the light oak-birch community on the now truncated soils, and the alder on the gley which gave such good hay land. This movement was the expansion of the *gafaelion*, when

morcellation became extreme, a process of assarting which continued under the name of squatting into the last century. There was a marked expansion in the sixteenth century, when cattle became important, and many summer houses (*hafodtau*) used in transhumance became permanent holdings. The late nineteenth century saw some recession, when many small holdings were abandoned for the alternative of mining.

Now it is quite impossible to lump these stages together and relate the whole to the physical background (14). The relationship is different at each stage, and each stage should be considered separately. For example, in the first stage settlement was on an easily defensible spur, and probably related to a mainly pastoral economy with sturdy oaks in the valley forming a relative limit while the light oaks on the now truncated soils were gradually cleared for a little arable. In the fifteenth century we are at an entirely different stage. The gleys are cleared for expansion as well as the oak, and alder is being cleared for hay on the valley rim. The limit has been pushed still further by the last century, when pressure of population dictated the working of the moorland. To-day sees a recession to a lower limit, with transport affecting the concentration of settlement along the roads. It is necessary to think in terms of such stages, at which physical factors operate differently, in all settlement studies. The given, physical background is more or less static, except where changed by man : the society is dynamic. Looking at the entire series of stages, therefore, it is difficult to escape the conclusion that the limits are man's limits and not nature's : the changes in the inter-relationship depend upon changes in the dynamic component. Granting an absolute limit which is physical, but which rarely operates in Britain, we are dealing with what Forde called a ' geographic circumstance ' (15), and a circumstance is relative to changes in any one factor involved. The limit is placed by circumstance ; in this case one which can be altered by varying cultural stages. When standards of living were low in the Conway Valley, and morcellation was great, the limit was nearer the river. New conditions brought expansion—and new limits, imposed by the society and its needs. This kind of geographical limit is very much like an horizon, discernible enough, but depending upon the extent of vision ; a change in height sees an extension of the horizon. In fact the horizon does not exist in nature, but in the position of the observer. So the limit of settlement in this case is determined less by nature than by the cultural stage of the people.

To sum up. Firstly, there are several aspects of settlement which must, for clarity and analysis, be studied separately in relation to the numerous factors involved. In the example quoted it is clear that pattern is the result of cultural forces. Siting involves microphysiographic factors ; distribution in terms of limits may be an expression on the balance between physical factors and human needs.

Secondly, the study should reflect the dynamic nature of limits by examining all the possible cultural stages in historic sequence. This suggests that it is the cultural stage which determines the actual limit within those broader limits such as altitude which is brought into operation in Great Britain in extreme cases only.

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(14) Even in relation to pattern these stages are of great importance. Devolution implies a change from nucleated to dispersed. The problem is not a simple one of explaining nucleated *or* dispersed settlement in relation to physical background, or even to ethnic groups as Meitzen postulated. Miss Harris illustrated the fallacy of regarding a pattern as necessarily static (' Settlement and Field Systems in Guernsey.' 1930, *Studies in Regional Consciousness and Environment*, Ed. Peate, I. C. Professor Evans (*op. cit.*) showed the same type of late dispersion in Ireland as Miss Harris found in Guernsey.

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LECTURES TO SCHOOL SCIENCE SOCIETIES

The Council recently decided to offer to help the organisers of Science Societies in schools to find lecturers for their programmes. In the first six months after this offer was announced the office received applications for assistance from twenty societies. The request for help were varied but were mostly for advice about speakers and suitable introductions were made. From letters of thanks received it appears that this is a service which is greatly appreciated and likely to grow. The office would welcome additions to the list of people who are willing to lecture to schools and any members of the Association who are prepared to offer their services are cordially invited to write to the Secretary indicating the subjects and when they would be willing to talk and the region in which they would be prepared to travel. No fees are paid but most schools are able to meet travelling expenses.

BIOLOGY IN SCHOOLS

INTERIM REPORT on work done between December 1949 and June 1950 by a Committee appointed to consider the position of biology and rural science in the school curriculum and matters relating thereto.

Prof. L. J. Audus, *Chairman*; Dr. L. M. J. Kramer, *Secretary*; Mr. F. H. C. Butler, Mr. W. J. Heasman, Prof. C. T. Ingold, Prof. H. W. Miles, Dr. G. E. Newell, Mr. A. C. Penney.

THE Committee is essentially a fact-finding Committee whose main task is to collect information concerning the biological and rural science work of all types of school and related institutions.

General Considerations

Since a considerable amount of information exists about Grammar School Biology and in view of the problems confronting the newly constituted Secondary Modern Schools, it was decided that these latter should receive prior attention.

In view of the experience of similar committees, particular attention has been paid to the validity of the questionnaire as an objective research method. It was felt, in view of the lowering of barriers between subjects, especially in Secondary Modern Schools, that the framing of an objective questionnaire to schools presented great difficulties. The following two approaches were therefore made.

Circularisation of University Departments of Education

A letter was sent to the Departments of Education of the 24 Universities and University Colleges in Great Britain asking for information and comments on existing problems concerning the teaching of biology in schools in their particular area. Eighteen Departments have responded to date. The results are briefly summarised below.

(i) *Secondary Modern Schools.*—Biological work varies from a total neglect to a few very good courses based on gardening, etc. In the main hygiene with biology as an adjunct is taught, and there is generally some standard laboratory equipment available. Two adverse factors, however, are the grave shortage of keen, well qualified teachers and an almost universal neglect of field work, even in rural areas. Laboratory work is also neglected.

(ii) *Secondary Grammar Schools.*—Although accommodation and equipment are satisfactory, courses are too 'bookish' and failure to exploit laboratory facilities to the full is very frequent. There is a serious shortage of well qualified biology teachers, and the number with pass degrees, or no degree, who teach the subject is increasing. Even where work is otherwise good, the nation-wide neglect of field work is an outstanding feature of the reports.

Circularisation of Teachers' Training Colleges

So far 49 Teachers' Training Colleges in Great Britain have replied to a request similar to the one sent to the Departments of Education. In 19 of these Colleges no biology is taught as such, but the remaining 30 have made reasonably full reports.

(i) *Entrants.*—About 10 per cent. of Training College entrants take biology as a subject. Only about one-half of these have Higher School Certificate in a biological subject. Both the quality and quantity of biology entrants are falling off, the latter being due, it is thought, to the increasing demands of industry for chemists and physicists. There is also evidence that students avoid biology since too academic a school training has prevented development of interest in the subject.

(ii) *Training in the Colleges.*—The main difficulties to adequate teacher training (mainly for Secondary Modern and Primary Schools) are :

(a) Two years is insufficient time (some Colleges suggest three years as a minimum).
(b) Lack of laboratory staff for practical preparations.
(c) Lack of interest of entrants.

(iii) *Position in Schools.*—Comments here are a complete confirmation of those from the University Departments of Education. They are :

(a) Grossly inadequate supply of trained biologists and the situation is rapidly worsening.
(b) Courses are too academic. There is a gross neglect of work with living organisms, due possibly to the fact that preparation of practical and field work requires so much more time and interest than for non-biological subjects. This is aggravated by large classes and lack of laboratory assistants.
(c) General science, when done, is almost always in the hands of non-biologists and biology is then omitted. In Secondary Grammar and Secondary Modern Schools accommodation and equipment are on the whole adequate, but are usually lacking in Primary and Junior Schools.

Comments

The major point arising from this work, which seems to require detailed consideration, is the method of training, in the Universities, of prospective biology teachers, both before and after graduation. It is felt that the vicious circle of the too academic approach must be broken in the Universities and that this can be done best by a much greater stress on field work. The greater emphasis apparent in the Universities in the teaching of practical ecology is therefore welcomed.

THE THEORY OF CONTINENTAL DRIFT

At the Birmingham meeting of the Association the Sections of Geology, Zoology, Geography and Botany held a joint meeting on September 1, 1950, to discuss the position of the theory of continental drift. Each of the six speakers in the symposium considered the possible validity, and implications, of the hypothesis from the view-point of a particular branch of science. The texts of these six papers are printed below.

The Case for the Crust-substratum Theory

by

Prof. J. H. F. Umbgrove

Those who have read the advance abstract of my paper will know that I do not belong to the protagonists of this theory. I feel like Mephisto who said about himself that he was the personification of the spirit who constantly denies. He also asserted that he *rightly* did so and I hope you will have a similar judgment about *me* after having heard my constant negation of the drift hypothesis. In speculations on continental drift the terms sial and sima are frequently used. The word sial was introduced by Suess as a *petrographic* term. Sima is another petrographic term created by Suess, mnemonic of its most abundant elements.

Wegener attached a special *physical* meaning to these terms. He simply declared that sial consists of rigid and elastic material in contrast to sima which he considered as viscous material in which the sialic continents were floating like wooden logs in a syrup.

In doing so he laid the foundation for his well-known theory of continental drift. Yet, there is not the slightest reason for thus arbitrarily endowing Suess' petrographic terms with two contrasting physical properties.

This basic assumption in explaining the mechanism of continental drift, appears to have satisfied Wegener and many of his followers. Yet, it is a well-known fact that basaltic material (sima) has a higher melting point, approximately 1,300° C., than granite (or sial), approximately 700° C. This means that the crystalline crust of the pool of sima must be as strong as or even stronger than the continental masses. Yet this crust must disappear somehow in front of an advancing continent.

The generally accepted view at present is that the earth is entirely encircled by a rigid elastic crust. It is further assumed that no sialic layer exists beneath the Pacific sector, though such a layer is in fact assumed to extend under the Atlantic and the Indian Oceans or at least under parts of these sectors.

This idea is supported by seismic and gravimetric evidence. Let us take one example.

Gravity observations over the Hawaiian Archipelago show positive anomalies which, even after reduction, still remain large (in the order of + 250 mgal.), whereas the anomalies in a station out in the deep sea in the neighbourhood are small. The positive anomalies on the islands become relatively small if a high degree of regional compensation is introduced. And this wide extent of isostatic compensation over an area with a radius of 170–240 Km. implies a rigid earth's crust of a large thickness. This result is valid for the Hawaiian Archipelago as well as for the Madeira area. The result therefore can be best explained by a down-bending of the crust under the load of the islands as the hypothesis of regional isostatic compensation assumes.

This result is important for our views on the migration of continents because it shows that in the present period such a movement would thus seem impossible.

But a proponent of the Wegener theory will say : All right ; granted that continental drift is impossible at present why should not the situation have been different in previous time ? Let us answer that objection by another example.

The Atlantic Ocean and the western

part of the Indian Ocean are characterised by a great number of deep-sea basins with comparatively flat bottoms surrounded by relatively steep ridges. The whole structure is in many respects very similar to that of the intervening African continent. The regions between the continental basins consist mostly of Pre-Cambrian rocks. Two main strikes have been dominant in Africa since the early Pre-Cambrian : a NE.–SW., the so-called Somali strike, and a NW.–SE., the so-called Erythraean strike. Both played an important part in the whole later history of the continent.

They are known as linear ridges separating basin-shaped depressions. Locally, and in certain periods, real fault-structures may appear, following the same direction. At present, for example, a line of volcanoes may be traced from the Cameroon fissure on the African continent oceanwards along the Guinea-ridge.

Whatever their mode of origin the planetary pattern of features which runs across the continents as well as across the ocean floors shows the existence of a rigid, elastic crust of considerable strength. The great antiquity of the shear pattern proves that the crust dates from the primeval times of earth history. From the moment a world-embracing solid and elastic crust was present continental drift was simply impossible.

So the features of the bottom relief at all events make it clear that, if the floors of said oceans originated as a result of continental sliding or stretching, this process must have occurred *during the early Pre-Cambrian.*

Another source of evidence is the contemporaneity of tectonic processes in widely separated regions of the earth as well as their rhythmic occurrence in geological time.

' If, on the other hand, the process of orogenic folding resulted from the drift of floating continents, no synchronism of even the major deformations need exist.' Yet, it *does* exist.

The crust-substratum theory finds further support in the distribution and time relation of volcanism and plutonism in orogenic belts.

A different form of converging evidence in favour of the crust-substratum hypothesis is furnished by the maximum altitude to which volcanoes may grow and by the plutonic cycles. And at the same time they are all in flat disagreement with the hypothesis of floating continents.

If an enthusiastic advocate of continental drift is not yet convinced, let us blindly accept a mysterious mechanism which allowed exactly the movement postulated by the drift hypothesis. Let us examine the consequences of the suggested drifting apart of the continental blocks. Even then we shall find that our experience is not in favour of the hypothesis of continental drift.

According to the displacement hypothesis the continental blocks have to be considered as pieces of a jig-saw puzzle. The main problem is the reconstruction of their original position.

Undoubtedly Wegener's Pangaea had the charm of simplicity and seemed to solve several heterogeneous problems. A great number of data show that the coasts of Australia, India and Africa including Madagascar cannot have formed a continuous whole in the sense of Wegener.

Mentioning these arguments is, however, hardly worth while since Du Toit grouped the blocks in a manner which differs fundamentally from Wegener's.

Du Toit placed the Antarctic continent in the centre of the present Indian Ocean. This is a rather safe procedure because hardly anything is known geologically of what is hidden below the ice-masses of Antarctica. So, testing the hypothesis by a comparison of opposite coast lines is made impossible. Moreover, even if such a procedure were possible the blocks are placed at a respectable distance from one another. Any discrepancy that might be found is therefore eliminated.

In a similar manner Du Toit leaves a lobby of some 400–500 Km., between Africa and South America.

It is well known that Du Toit not only pointed to the great resemblance between the Sierra and the Cape Mountains, but that he even tried to consider them as fragments of an original geosyncline which he tried to follow as far as Eastern Australia and New Zealand, the so-called *Samfrau geosyncline* extending at least 110 degrees in length.

Moreover, he simply placed the Falkland

Islands in the gap or rift between Argentina and South Africa. We are invited to imagine that the Falkland Islands would have moved south-westward during the westward drift of America. In this respect Du Toit was ' plus royaliste que le Roi,' for such an arbitrary suggestion of movements seemed to Wegener himself a too drastic procedure. Actually Du Toit's reconstruction of the Falkland Islands is untenable, for the fold belt of the Falkland Islands fades on West Island ; so it is certainly *not* a fragment of the jig-saw puzzle which fits in this place.

Evidently, however, this means at the same time a serious objection against the whole procedure of reconstruction of the so-called Samfrau geosyncline, the Australian and New Zealand part of which will be left out of consideration since the stratigraphy and structural history of these areas have not yet been examined in sufficient detail.

To return to the problems of the Atlantic Ocean : I think geologists have to face the fact that abruptly ending belts are indeed situated approximately opposite one another on either side of the ocean. The question is, can these features be explained if no process of drifting apart occurred since their formation ?

The following suggestions may help to overcome these difficulties. Let us, for example, suppose that, owing to the influence of subcrustal processes, the earth's crust had reached a stage where it became possible for a zone of subsidence (geosyncline) to form within a certain area. Let us also assume that this zone continued under an area such as the Atlantic. In that case a geosyncline and subsequently a fold belt would potentially be able to originate within a certain zone of the floor of the Atlantic. Yet, such a thing would never happen, for sufficient erosion products would not be available for the purpose of filling the furrow. Should a subsiding trough originate within this zone and be buckled, the result would be a sialic root of considerably smaller dimensions, for the sialic layer of the Atlantic is a much thinner one than that of a continent. Should this nevertheless happen, the isostatic anomaly would be much smaller than in the contemporaneous extension of this zone on the continents. This would help to show that a trans-oceanic connection need never have existed above sea-level, and that the stratigraphy, tectonic structure and epochs of folding of mountain-belts are more or less analogous on either side of the ocean in spite of the absence of trans-oceanic mountain-belts.

Now there are several reasons why we should prefer such a theory instead of accepting the hypothesis of continental drift. Suppose the American continents to have drifted towards the Pacific. We have to consider the fact that the ocean floor is blanketed by some 4 Km. of deep-sea sediments. Would not they have been squeezed and piled up like ice in front of a square-nosed ship ?

Taking the westward drift of South America at 3,000 Km., 1,500 Km.[3] of deep-sea oozes would have been piled up in front of each kilometre of the western coast. After complete isostatic compensation at least 1,000 Km.[3] would still emerge out of the sima. Such a mass would form a plateau 200 Km. broad at sea level. Actually the continental slope is one of the steepest of the world and is fronted by deep-sea troughs.

And what would be the result of drift in the Atlantic Ocean ? If the Atlantic originated in comparatively recent times as a result of continental drift we certainly would expect to find much thinner strata of deep-sea sediments. The facts, however, are not in accordance with this hypothesis. Petterson, the leader of the Albatross expedition, mentions that depth-charge soundings of the thickness of the carpet of sediment gave a maximum value of nearly 10,000 ft. in the Atlantic, representing a time-span of 300–400 million years, possibly still more.

Let us now turn our attention to a few other fundamental aspects of the displacement hypothesis.

The island arcs along the eastern side of Asia would have originated in front of a continent which was considered as either stable or advancing in their direction. Conversely, the island arc between South America and the Antarctic continent was explained as a string of islands which lagged behind during the westward drift of America. In the case of the East Indies the island-arcs would owe their origin to

the northward drift of Australia. These different modes of explanation for the same type of earth-features bear the marks of inconsistency and opportunism instead of scientific investigation.

In the opinion of protagonists of the theory of wandering continents the mere fact that there have been sweeping changes in the positions of the climatic girdles from the Paleozoic onwards, would alone serve to prove a wholesale crustal creep, from which continental drift would follow automatically.

However, this opinion is based on a fundamentally wrong interpretation of the climates of the past by Köppen and Wegener.

They undoubtedly lost sight of the abnormal character of actual conditions when determining the climatic zones of former geological periods, for they sought to reconstruct climatic zones equivalent to our own for all these formations, and any conflicting evidence in the past was simply attributed to hypothetical changes in the position of the poles and continents. They thus postulated a continuous, non-periodical process, instead of the periodicity of abnormal climates.

If noted in a proper order on the geological time-scale, periods of glaciation and periods that were characterised by a mild climate up to high latitudes will be seen to give rise to a curious phenomenon, viz. a large climatic rhythm. And a remarkable feature is that it shows relations with the rhythm of mountain building. Abnormal climates may be explained by the combined influence of widely diverging factors, among which, however, the shifting of poles and continents has played no part.

Whether the hypothesis of continental drift be accepted or rejected is a question of geology and geophysics. If it is rejected on geological and geophysical grounds problems of biogeography ought to find a solution along different lines of speculation.

The most prominent problems of biogeography have been explained by various able and outstanding biologists and palaeontologists without the acceptance of drifting continents. Yet some biologists and palaeontologists may think there are facts which force them to accept trans-oceanic land-connections in the geological past. What can geological science offer them in this respect?

Among the features which are liable to change in one or perhaps several respects are the details of the submarine relief. For example the ' rejuvenation ' of the relief, which is so strikingly illustrated by the Mid-Atlantic Rise and the Carlsberg System. These movements remind one at once of the ' isthmian links ' of Schuchert and Willis. I consequently incline towards the view that no essential objection can prevent us from accepting the idea of isthmian links as postulated by Schuchert and Willis. The existence of land-connections during certain given periods cannot be proved geologically but such land bridges need not be discarded *a priori* as mere products of the imagination.

Wegener considered the possibility that his sialic block Pangaea formed from an original world-embracing sial layer. Why and how the sialic layer became heaped up into one block is not made clear, nor why the Pangaea block subsequently split up in several sial flakes. Nor is their present distribution explained.

Most of the continental parts are antipodal to oceanic sectors ; only 6 per cent. form an exception. This more or less tetrahedal symmetry in the earth's major topographic features appears to be governed by equations in spherical harmonics in which especially the orders 3, 4 and 5 play a dominating part. In a recent paper Vening Meinesz made clear that this can be explained by assuming that convection currents have been responsible for the distribution of continental masses. The horizontal drag exerted by such currents on a primeval world-encircling sial layer would have swept the sial together towards those areas where the currents descended.

As mentioned by Vening Meinesz this explanation of the position of continental and oceanic sectors provides a strong argument against Wegener's hypothesis of continental drift. For a subsequent drift of the continents would have destroyed a spherical harmonic distribution.

So, remarkably enough, the same idea, viz. a world-encircling sial layer, is taken as the starting point of both the hypothesis of continental drift and the crust-substratum theory.

The fundamental difference is the time in which the movements of the sial flakes

are thought to have taken place. Wegener's Pangaea broke into pieces which moved towards their present situation during the last 200 million years, and the Atlantic, for example, would have originated in the last 100 or 60 million years, whereas several arguments show that continental drift, *if* it ever occurred, must have taken place in early Pre-Cambrian times, some 3,000 million years ago.

The Distribution of the Flowering Plants in Relation to Theories of Continental Drift

by

Prof. R. D'O. Good

My remarks concern one group of plants only, the Flowering Plants or Angiosperms, and the significance of this requires a word of preliminary explanation.

The Angiosperms hold, in the plant kingdom, the same relative position that is held by the mammals in the animal kingdom. As far as we know both arose about the same time, somewhere in the Mesozoic, and are thus the latest and most highly evolved members of their respective kingdoms, with what, in comparison with many other groups of organisms, is a relatively short history.

There are other interesting comparisons between the groups of the Angiosperms and the Mammalia, but more important here is one of the prominent differences between them, namely their size. There are, I believe, about 3,000 species of mammals but a recent review of the Angiosperms credits them with a total of more than 225,000 species, or seventy-five times as many. This vast assemblage moreover constitutes, quantitatively, practically the whole of the vegetation of the land surfaces of the globe, with the single significant exception of the northern coniferous forests, and almost invariably forms the substratum on which the rest of life is borne. In this sense at least the Angiosperms may be regarded as the fundamental biological expression to-day and their speed of evolution and spread must have been very great.

It is from the point of view of their spread that they are of special interest in problems of continental drift. Unlike all other plants except the Conifers and their allies, the Angiosperms reproduce by seeds which are comparatively bulky structures much less easily disseminated than the far simpler spores of the lower groups. This, coupled with their shorter existence, makes the geographical history of the Angiosperms a matter of quite different values and not necessarily to be compared with that of any other plants. Particularly does it seem that their distribution to-day is simple in the sense that it illustrates the results of a single progressive colonisation of the world's surface, and it seems reasonably safe to assume that these plants have not had time to carry out the same course of migration on a large scale more than once.

Although this makes for simplicity, the multiplicity of the Angiosperms raises difficulties of its own. On the one hand the group is so large that it is hard to comprehend the whole of it simply; while on the other it is so all-embracing that it is nearly always possible to find within it, by diligent search, any particular state of affairs. Not unnaturally too with so huge a group much of its more phyletic classification is far from satisfactory. In matters geographical particularly this last point is most important because unless it is reasonably certain that a group of organisms is a truly natural and related one, it is, to say the least of it, unwise to draw too many conclusions from its distribution.

In short, there is a danger, in using plant geographical data in such connections as that under discussion to-day, of arguing too easily from the particular to the general and I have therefore tried in this contribution to set out what seem to be the leading general features of Angiosperm distribution, namely the broad facts which must, in general, be accounted for in any theoretical explanation of it.

These leading features may be arranged in two series, one of eight general or major points, and one of eight regional or minor points. The actual number of points has no special significance nor is their order necessarily that of their importance, though the earlier ones naturally include the more fundamental. Unfortunately time does not permit of elaborate illustration of all these statements, but brief comments are made on most of them.

The following are the general points :

1. *The distribution of Angiosperms may be regarded as complete and these plants are nearly everywhere the dominant group on land.*

Whatever their history may have been in detail and whatever the vicissitudes they may or may not have suffered the Angiosperms have to-day spread to every corner of the habitable world in considerable numbers and are predominant everywhere except in coniferous forest.

2. *There is an essential unity between the floras of different parts of the world, and this is specially exemplified by the constant proportions of Dicotyledons and Monocotyledons (the two great groups into which the Angiosperms are divided).*

Everywhere in the world the floristic make-up is fundamentally the same. Nowhere is there a flora peculiar to itself and composed entirely of major elements quite unrepresented elsewhere. The order, by size, of the larger families is very much the same everywhere and the grasses and composites are very generally in the lead.

The fact that in nearly all floras there are from two and a half to four times as many Dicotyledons as there are Monocotyledons is well known and is of great interest, and its implications merit careful attention.

3. *There is a conspicuous absence of very large endemic groups confined to one particular portion of the world.*

If an endemic family is defined as one confined to the limits of a single continental or subcontinental mass or an equivalent area, then about 20 per cent. of Angiosperm families are endemic, but of these only three, the Alstroemeriaceae, Selaginaceae and Ruscaceae, are more than very small. The nearest approach to large endemic groups is the family Cactaceae, with well over 1,000 species and confined to the New World.

4. *Both the density of species concentration and the proportion of endemic species vary considerably in different parts of the world.*

The great numbers of Angiosperm species are by no means evenly distributed over the globe. Rather naturally there is a steep diminishing numerical gradient towards the less hospitable polar regions but this gradient is by no means smooth.

The problem of specific wealth, or the density of species number per unit area, is another of peculiar interest but also of many difficulties, and all that can be said usefully here is that, *for their size*, the Cape Province of South Africa, the Balkans, Borneo, the Philippines, the Malay Peninsula, Cuba and New Caledonia are among the areas unusually rich in species. Smaller oceanic islands have far fewer species than most continental areas of the same size. At the other end of the scale tropical Asia and tropical America are much richer in species than tropical Africa.

The number and proportion of endemic species vary enormously. Some places, as for instance Great Britain, have virtually none and from this condition anything up to 90 per cent. of endemism, the figure now quoted for the Hawaiian Islands, may be found. It should be added that the proportion of endemic species among Monocotyledons is less than among Dicotyledons which means that the average distribution of Monocotyledon species is greater than that of Dicotyledons.

Neither species wealth nor degree of endemism is a simple function of latitude or longitude.

5. *Generally the floristic change from country to country is gradual but there are some conspicuous instances of the reverse.*

In some parts of the world the flora changes in the course of a comparatively short distance to a degree which cannot be attributed solely and entirely to change of conditions, so that these places seem to mark the boundaries of one great flora with another. Such places and changes have been called ' demarkation-knots ' and the full elucidation of them is one of the most promising researches in relation to palaeogeography. Good examples are found bordering the Malaysian region and have been commented upon by van Steenis in the first instalment of the new *Flora Malesiana*.

6. *Discontinuity or disjunction, which is the presence of a species or group of species in two or more widely separated areas, is of frequent occurrence.*

This is the feature of plant geography which has attracted most attention from those interested in theories of continental drift and on that account has become something of a commonplace. It is therefore important to remember that discontinuity is not an inevitable characteristic of all possible circumstances but that its occurrence does mean one kind of

evolutionary and geographical history rather than another.

Discontinuity is also a subject very liable, for reasons which have been explained elsewhere, to misquotation, and the statements about it should be used with great caution. For the same reason it is difficult to arrive at any quantitative estimate of its frequency but more than 60 families of Angiosperms, mostly small ones, and at least 5 per cent. of all the genera are notably discontinuous.

There are discontinuities in practically every direction but those between the isolated parts of the tropical zone are most numerous.

7. *There is a wider morphological range in the tropics than elsewhere, expressed in the fact that more families occur within the tropics than outside.*

Practically all the families of Angiosperms, except a few of the smaller ones, are represented in the tropics, if only on the mountains there. The familiar northern temperate families are nearly all widely, and often predominantly, found in the tropics and there is no single large family which can be strictly described as exclusively temperate. The Cruciferae are probably the nearest approach to this, followed by the Umbelliferae, the Caryophyllaceae and Ranunculaceae. It is noteworthy that the first two are closely related to well-known tropical families. There are only about half a dozen very small families strictly confined to the northern temperate regions.

8. *There is increasing floristic diversity from north to south.*

This is in conformity with the similarly increasing geographical isolation, and may be expressed by saying that the arctic flora is virtually one throughout ; that the north temperate flora divides essentially into Old World and New World parts but with many similar or identical forms in both ; that the tropical and subtropical flora, though with many common elements, consists of three parts each with a character of its own ; and that the southern floras are markedly different from one another.

Regional or minor features are :

1. *There are some continental areas with unusually high degrees of endemism.*

Examples are the Mediterranean ; the Cape Province ; south-western Australia ; and California.

2. *There are some insular areas with unusually high degrees of endemism.*

Examples are Madagascar ; New Zealand ; New Caledonia ; and Hawaii.

3. *There is an absence of any strongly marked south Pacific flora, namely south of Hawaii and east of New Guinea, New Caledonia and New Zealand.*

The andesite line does not appear to be a line of notable botanical demarkation.

4. *There is no comparable west-Eurasian counterpart of the warm temperate floras of south-eastern North America and south-east continental Asia.*

This is a somewhat more general expression of one of the most familiar facts of plant-geography, the floristic relation between Atlantic North America and far-eastern Asia.

5. *There is no flora in temperate South America comparable in richness and endemism with the floras of the Cape Province, south-western Australia and New Zealand.*

6. *There is a notable floristic relation between temperate South America and Australia-New Zealand.*

This is another well-known feature of Angiosperm distribution and there is an extensive literature relating to it in which the writings of Skottsberg are prominent.

7. *There is a notable floristic relation between South Africa and Madagascar on the one hand and Australia and New Zealand on the other, and more particularly between South Africa and Australia.*

8. *Some desert and aquatic plants have particularly wide distributions.*

That this is true of many aquatics is well known. That the same is true, within their limits, of various desert plants, such as some members of the Zygophyllaceae, is less familiar.

These 16 points are, it seems to me, the important ones which must be taken into account by those who would employ the criteria of Angiosperm distribution in scientific argument. The question is—to what palaeogeographical conclusions, if any, do they point ?

There is general agreement that it is difficult to account for them without supposing that the isolation of the chief land masses has, at some time or times in

the past, been less than it is to-day, and the problem is really what form this difference may have taken. It is a case either of land bridges or of some form of continental displacement or shifting.

I have tried to confine myself to the recital of what appear to me to be the salient facts irrespective of theories of continental drift, but as a final and more direct contribution to this discussion I would like to make one comment.

I am often struck by the combination of the simple and the puzzling in plant-geography. The most troublesome problems seem to be superimposed, as it were, on a more rational and straightforward basis. It is difficult to avoid the belief that the explanation of this has something to do with the time factor and that if we understood the order of events more perfectly many difficulties would disappear. A common expression of the theory of continental drift postulates that the sundering of various continental masses began in a relatively remote geological period and that it has continued without notable cessation ever since. The distribution of Angiosperms does not seem to be in special accordance with any such particular course of events. It suggests more strongly that sundering occurred after these plants as a group had become well diffused, let us say, by the Eocene. Similarly I know of nothing in the distribution of Angiosperms to show that drift is still going on.

Can it be that continental drift has in fact been, not a persistent process, but an intermittent feature of geological time, as for instance have the great glaciations? If so, and if it can further be shown that the most recent of these 'drift ages' took place not earlier than the beginning of the Tertiary, then at last the plant-geographer will have to hand a master-key to most of his perplexities.

The Wegener-Du Toit Theory of Continental Displacement and the Distribution of Animals

by

Dr. H. E. Hinton

The former existence of some land bridges that permitted the migration of terrestrial animals between continents now separated by shallow seas is admitted by everyone. Nobody denies the existence of an effective Bering Strait bridge in former times; and there is general agreement that there was no land bridge between North and South America in the Permian and that such a bridge was present from the late Cretaceous to the early Eocene and then disappeared until about the mid-Miocene. Land bridges such as these result from relatively slight alterations in the relation of land and sea, such as those caused by minor vertical movements of the land masses or by the withdrawal of water during periods of extensive glaciation.

Isthmian bridges, such as the Panama bridge connecting North and South America, do not differ in principle from the land bridges that are now known to have connected some of the islands standing on the continental shelves to their respective continents. Much is now known of the shelf bridges that connected one or other of the West Indies with Central and South America at various times in the Tertiary (1). The relations of land and water in the East Indies during the Pleistocene are now well known. For instance, from the distribution of submerged coral-reefs (2) and former river-channels beneath the sea, it is known that the Pleistocene glaciations in the Northern Hemisphere had the effect of lowering the sea-level from 100 to 200 metres. The 100 and 200 depth lines are close together, and a drop in the sea-level of this order had the effect of uniting the Malay Peninsula with Borneo in a continuous land mass separated by no stretch of more than 100 miles from the Australian continent.

In addition to shelf bridges connecting continents with each other or islands to continents, most biogeographers in Europe and a small minority in the United States agree that the facts of distribution require direct land connections between continents that are now separated by deep oceans. Until fairly recently, biogeographers usually visualised these connections as bridges, often of continental size, elevated from the ocean floors. A land connection between South America and Australia was proposed as long ago as 1847 by J. D. Hooker; and during the latter half of the nineteenth and first part of the twentieth centuries the number of

such bridges postulated by an ever-growing body of biogeographers was immense. Bridges were erected not only across the young Atlantic and Indian Oceans, but by many also across the Pacific ; and such was the lack of restraint that were all to be plotted on a map most parts of the oceans would be covered by a close network of bridges throughout the Mesozoic and much of the Tertiary. In the fullness of time, owing largely to the efforts of the more responsible biogeographers, it was gradually accepted that the facts of distribution required relatively few inter-continental connections, the most important of these being between Brazil and Africa and between Australia and Antarctica. A comprehensive review of this subject is given by Arldt (3).

Factual and theoretical objections to the idea of former inter-continental bridges soon began to make themselves felt, and in time the idea that such bridges had existed was finally abandoned by most workers in favour of the doctrine of the permanence of continents. A bridge like that postulated across the Atlantic between Brazil and West Africa must have been very broad and must have contained freshwater streams of considerable size if it were to be effective in permitting the transfer of numerous plants and animals with very different environmental requirements. Such a bridge, about 5,000 miles long, means that a block of continental size must be postulated. Even allowing for the North Atlantic Rise, most of the 5,000 miles would have to be elevated as much as 12,000–15,000 ft. Any displacement of the sea of this order would inevitably lead to the submergence of much of the land areas of the already existing continents and so drown the very populations that the bridge was supposed to join. Of course, it may be suggested that the emergence of such continents would be feasible from a biological point of view if the volume of water in the oceans had been increasing by a few cubic miles each year since the Mesozoic. There is, however, no evidence to support such a theory. A further objection to the past existence of transoceanic bridges, which involves accepting sunken continents as a necessary corollary, is the nearly complete absence of deep-sea deposits on the existing continents, since we would expect some of the latter to have been also elevated from the ocean floors. A few geologists, for instance Umbgrove (4), still please themselves by erecting bridges of continental size but refrain from dealing with the now classical objections to such erections.

Once the theory of the permanence of continents was accepted, biogeographers were split into two sharply opposed rival camps. To the first camp belong the American vertebrate palaeontologists Matthew (5), Simpson (6) and others who claim that the facts of distribution are consistent with dispersals possible if the spacial relations of the continents were always what they are now. The only land connections that they postulate that do not now exist are isthmian and other shelf bridges. They freely evoke accidental wind-borne or raft-borne transportations across the oceans to account for distributional patterns that do not fit their theory.

To the second camp belong the majority, at any rate in Europe, who claim that in addition to isthmian or other shelf bridges, and accidental transportations across the oceans, the facts of distribution can be satisfactorily explained only by assuming direct land connections between continents now separated by deep oceans. Those who belong to this camp claim that there must have been effective and direct land connections between Brazil and Africa and between Australia and Antarctica that were not seriously interrupted until the late Mesozoic or even Paleocene.

The only theory that has the support of a large number of geologists and some geophysicists, and that at the same time provides the continental connections required by biogeographers, is the Wegener-Du Toit theory of continental sliding. It is for this reason that most modern biogeographers accept the broad outlines of the theory of continental sliding and obviously not because they believe that distributional patterns are direct tests of geophysical or mechanical possibilities. Theories of continental sliding brought about by sub-crustal convection currents (7) that take into account the thin sial sheet of the Atlantic and Indian Oceans meet with no objections from biogeographers, as biogeographers, unless it is claimed that

this sliding could only have occurred long before the end of the Mesozoic. For instance, Umbgrove (*op. cit.*, p. 231) claims that if such sliding took place it could only have been before the Cambrian. Here, clearly, is a variation of the sliding theory we cannot accept.

It cannot be overstressed that once the possibility of continents sunk to the ocean bottom is ruled out, all of the biogeographical arguments in favour of the transoceanic bridges previously mentioned are, in effect, arguments in favour of continental movements of the degree postulated by Wegener, Du Toit, Holmes, and those geologists who agree to some form of drift or sliding in the Mesozoic.

It is of some interest briefly to consider the anti-drift position adopted by a few such as Matthew, Simpson and their followers, since within the field of zoogeography the most serious opposition to the theories of drift come from them. No one can seriously dispute the fact that Matthew (5) fully established his contention that many Tertiary groups of mammal have dispersed from centres, chiefly in the Holarctic Region, to the ends of the southern continents. But according to displacement theories, the effective separation of the continents had already occurred by the middle Eocene. For this reason I see no necessary conflict between most of Matthew's arguments and theories of sliding. Even so, Matthew time and again falls back on accidental transportations across the oceans. His explanations of the discontinuous distribution of mammals, so convincing for post-Paleocene groups, meet with great difficulties when applied to earlier groups. For instance, the presence of Hystricomorph rodents in South America and Africa, and their absence in the Tertiary of North America, has forced Matthew to suggest that they were accidentally carried by raft from Africa across the South Atlantic to South America. A theory that rests on an assumption as unlikely as that of a rodent surviving a raft journey of some 5,000 miles hardly requires serious consideration. Cuba, for example, is in one of the most favourable positions for this kind of invasion, yet no terrestrial mammal has ever invaded it from North America. Cuba is about 600 miles long, only 200 miles from the Florida coast, and about 400 miles from the mouth of the Mississippi. Furthermore, any rafts brought into the sea by the Mississippi are likely to be swept close to the Cuban coast by the Caribbean Current.

The possibility of a raft invasion of Hystricomorphs across the South Atlantic is not now generally accepted. To those who will not accept displacement theories only one possibility is left, viz. they must reclassify the group and show that the New and Old World Hystricomorphs are not related but that their resemblances are due to parallelism. An attempt of this kind has recently been made by Wood (8), who says (p. 90), ' The fourth hypothesis, that the South American group has been derived from non-hystricomorph North American ancestors independently of the Old World hystricomorphs, has never been extensively championed in print. Such a hypothesis, however, would solve all the problems of distribution, and would be much the most preferable if it were supported by morphologic (*sic*) or palaeontologic (*sic*) evidence.' Wood then proceeds to justify this hypothesis in a fashion that is perhaps a little unconvincing to himself and certainly to me. Wood concludes his paper as follows : ' If the similarities between the Old and New World hystricomorphs are entirely parallelisms, it leaves the taxonomic problem of what to call these groups and how to arrange the classification of the order. This problem cannot be settled at the present time, as it should be taken up in connection with a re-study of the classification of the entire order.'

The chance of accidental dispersal varies according to the nature of the group. Colonisation of the Hawaiian and other volcanic islands, always far removed from any continent, provides us with absolute proof of the kinds of animals and plants that can withstand long-distance wind or raft transportation across the oceans. At the same time, the oceanic islands provide much less certain evidence of the kinds of animals and plants that will not survive this sort of transportation. When we consider the discontinuous distribution of any group, the evidence for long-distance accidental dispersals must first be determined. For instance, if the

group occurs on any of the oceanic islands, and there is no evidence that it has been introduced there by man, its pattern of distribution cannot be used for or against theories of continental displacement. I mention this only because some writers, e.g. Longwell (9), cite the discontinuous distribution of groups known to occur on oceanic islands as evidence against continental displacement, which is absurd.

One of the best informed and most powerful attacks on Wegener's theory from the field of zoogeography is that of Simpson (*op. cit.*). Du Toit (10) (11) successfully deals with most of Simpson's arguments but not with that of the presumed lack of resemblance between the South American and South African Triassic reptiles. The latter argument has been accepted by geologists, even those who favour Wegener's theory, e.g. Holmes (*op. cit.*, p. 444). Simpson's argument is based on faunal resemblances, and these are his figures :

	A	B	C	D	E
Age . . .	Recent	Recent	Pliocene	Pliocene	Triassic
Distance . .	500 m.	3,300 m.	5,000 m.	6,000 m.	4,700 m.
Families .	100 %	67 %	61 %	4 %	43 %
Genera . .	82 %	24 %	15 %	0 %	8 %
Species . .	65 %	5 %	0 %	0 %	0 %

(A) Per cent. of recent Ohio mammals also found in Nebraska.
(B) Per cent. of recent New Mexico mammals also found in Venezuela.
(C) Per cent. of known early Pliocene North American mammals also found in the early Pliocene (Pontian) of Europe.
(D) Per cent. of known Pliocene North American mammals also found in the Pliocene of South America.
(E) Per cent. of known Triassic South American reptiles also found in the Triassic of South Africa.

Simpson claims that, ' these figures are decidely inconsistent with any direct union between South America and Africa.' He admits, however, that the resemblance of the reptiles of the two continents was greater then than now. I claim, on the other hand, that not only do his figures show the exact opposite of what he claims, but that they constitute one of the best zoogeographical proofs of the displacement theory.

Simpson claims that the distance between these Triassic reptile faunas is 4,700 miles. This, from him, is a truly remarkable statement, since he is much opposed to transoceanic connections of any kind. On his own theory, therefore, this distance is in the neighbourhood of 13,000 miles.

Column D shows the agreement between North and South American Pliocene mammals. The very low percentage of similar forms is only to be expected when we remember that for mammals the Panama bridge was available only from the late Cretaceous to the early Eocene and again for a very short time before the Pliocene. If we turn again to E, we see that the similarity of South American and South African Triassic reptiles is very much greater, notwithstanding the fact that they too (if Simpson is right) had to use the Panama connection. It is even more astonishing when we realise that there was no Panama bridge in the Triassic and Permian, and that the distance for the reptiles is more than twice as great.

We may now consider column B for a moment. One hundred per cent. of the recent families of New Mexican mammals must be known and at least about 98 per cent. of those of Venezuela. The direct route here is only about one-tenth that of the Triassic reptiles we have been considering. In addition, the Panama bridge has been available for mammals for about the last 15 million years. Now, nothing like 90 per cent. of the Triassic reptiles are known, and so, for another reason again,

we are struck by the figure of 43 per cent. given by Simpson. In comparing these figures it must be remembered that for rapid dispersals most of the advantages are with mammals, since they are much better able to deal with climatic variations.

If we accept the displacement hypothesis, however, the similarity of the Triassic South American and South African reptiles is comprehensible, and the distance between the pertinent fossil beds is then only about 1,300 miles.

The discontinuous distribution of particular groups of animals and plants cannot of itself be certain proof of former land connections. For instance, a terrestrial or freshwater group of pre-Tertiary origin confined to South America and Australia suggests the existence of a former connection between Australia and Antarctica but is not, nor can be, sure proof, since it can never be finally demonstrated that it did not also occur in other regions in former times. If, however, identical fossil forms occur in different continents in identical beds in a complicated and homologous series of strata, the evidence of former connection is much stronger.

Of more significance than the kind of similarities mentioned in the preceding paragraph, is the fact that it is precisely the Australian and South American groups that tolerate only a very narrow range of environmental conditions, and apparently cannot be accidentally transported, that are most alike. A very high proportion of the terrestrial insects of southern South America show some relation to those of Australia, but those that require running freshwater for survival, e.g. the beetle family Elmidae, are still more similar to the Australian forms than terrestrial groups that tolerate a wider range of conditions and are generally more rapidly dispersed.

A great deal of zoogeography has unfortunately been done by merely collating catalogue names. It should be evident that the more we know of the conditions of life required by the animals, the more acutely will we be able to distinguish between what is and what is not significant in their distributional patterns.

One of the strongest, if not the strongest *single* piece of geological evidence for Wegener's theory is provided by the distribution of Permo-Carboniferous glacial deposits. These are found in Africa from the Cape to slightly above the equator, in South America from Argentina nearly to the equator, in India, in New South Wales and in Alaska. To understand the full significance of this distribution, it must be realised that in Europe, China and North America there was at the same time a tropical or semi-tropical climate in which coal deposits were being formed. If the continents were in their present latitudes in the Permo-Carboniferous, the existence of an immense ice-sheet on the equator must be accepted. But if that were so, life on this planet would have been practically extinct. The existence of an immense glacial sheet on the equator with semi-tropical or tropical conditions in the Northern Hemisphere is, as pointed out by Simpson (12), a conjunction of climates quite impossible from a meteorological point of view. This apparent contradiction between well-established facts is only resolved by assuming that the areas bearing the glacial tillites were near the South Pole, as they are in Wegener's reconstruction of the Permo-Carboniferous. Furthermore, the alternation of coal deposits and glacial tillites known to occur in New South Wales is conceivable only with a relatively limited glaciation of the Southern Hemisphere undergoing periods of recession and expansion such as are known to have taken place in the Northern Hemisphere during the Quaternary glaciations. The presence of glacial tillites both in Alaska and in the Southern Hemisphere in the Permo-Carboniferous, contemporaneous with the coal deposits of middle latitudes, seems to me evidence that during the Permo-Carboniferous the poles were in their present positions.

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Mechanical Aspects of Continental Drift and Alternative Theories

by

Prof. Harold Jeffreys, F.R.S.

There is a story of Edgar Allan Poe which relates to an important stolen letter. Numerous searches failed to find it, but at last it turned out that the thief had simply stuck it in the letter rack—that being the obvious place to put a letter, nobody had thought of looking for it there.

Discussions of the mechanics of the Earth's crust show a similar psychological phenomenon. We have all learnt at school that fluids tend to settle down with level surfaces, and solids do not, and that the reason why the Earth's rocky surface does not settle down into a spheroid, uniformly covered by the ocean, is that it is solid. The main difference between a solid and a liquid is that under a constant shear stress, however small, a liquid keeps on flowing, whereas a solid undergoes only an elastic deformation, unless the stress is too great, when the solid breaks. This simplifies the behaviour of solids a little; in particular fracture is often accompanied by a certain amount of bending. But *prima facie* we can expect to get a good idea of the properties of the Earth's crust by treating it as a solid, undergoing only elastic deformations up to a certain shear stress, which we will call the strength, but breaking if that stress is exceeded.

That natural hypothesis does in fact explain a lot, and I must insist that it does so because there are so many accounts that omit all mention of its successes. It turns out, for instance, that the strength needed to support the weights of the highest mountains is in good agreement with the strengths of igneous rocks measured in the laboratory. It explains automatically the intermittence of mountain formation; stress increasing gradually through geological time would give no fractures until the stress reached the strength, when fractures would occur and spread until most of the stress was relieved. It leaves plenty of room for widespread vertical movement; all that is needed is that radioactivity should not be uniformly distributed with respect to position on the surface. Earthquakes, comparable in intensity with the largest normal earthquakes, occur to a depth of a tenth of the radius, indicating that shear stress comparable with the strength of surface rocks can develop at those depths. The Pacific mountain system is a natural consequence of any theory of mountain formation, if we suppose, as is otherwise likely, that the ocean floor is stronger than the continents.

The alternative opinion that the Earth has no strength, that is, that it behaves like a liquid under long-continued stress, is the basis of the continental drift theory and of many later 'convection' theories. It had some support originally from the phenomenon of isostasy and the existence of tidal friction, which was originally believed to be internal. But the evidence from isostasy was shown by Joseph Barrell in 1912–13 to be incomplete. He showed that the data were in fact consistent with a decrease of strength as we go downward, but there was no evidence that it ever disappeared—in fact there were very definite signs, even in the very data that had been used to support isostasy, that it did not. Later work has fully confirmed his results. The tidal friction argument broke down when it was shown that turbulence in shallow seas is enough to account for tidal friction, and that bodily tidal friction, if adequate, would damp out in a few days the periodic motion of the Earth's axis known as the variation of latitude. There is no positive evidence whatever for absence of strength (or for a finite viscosity, which means the same thing) until we get to the core, nearly halfway to the centre; and there is plenty of evidence for its presence, at

least to the depth of the deepest earthquakes.

Estimates of viscosity have been derived from the rate of the Fenno-Scandian uplift. They are of the order of a million times Wegener's. But even they are under suspicion. They start with the assumption that exact isostasy was attained during the glacial period, and that the present uplift is a delayed response to the lightening when the ice melted. This might be true, but it can be tested further. Viscosity has no memory—if a region is abnormally light *now*, it should be rising, and any abnormally heavy one should be sinking. This is contrary to the facts. There are instances (e.g. Cyprus and the west of Scotland) where the opposite has taken place. There are still more cases (e.g. India) where systematic gravity anomalies greater than the Fenno-Scandian ones are associated with no measurable change of level at all. The facts are against a general application of viscosity, but perfectly consistent with the hypothesis that the changes of level are due to spontaneous changes of density in a strong substratum.

Wegener's theory started off with the hypothesis of viscous yielding, but he made no serious effort to work out its consequences. The decrease in strength downward was usually attributed to rise of temperature, but Wegener cheerfully took it to be due to an essential property of the material. Consequently he took the viscous rule to apply right up to the ocean bottom ; and this flatly contradicts the fact that the ocean bottom is not flat—its elevations and depressions are as great as those within the continents. At equal temperatures, under experimental test, basic rocks are stronger than acidic ones. Granting, as we must, the existence of strength in the ocean floor, we cannot have continental drift unless an adequate force to overcome it is provided, and unless such a force would produce the kind of motion contemplated in continental drift if it was provided. Considering that the theory has been advocated for 35 years, it is remarkable that nobody has suggested one of the right amount or even in the right direction. There is a gross mistake in Wegener's treatment of the Eötvös force—a force towards the equator, with a high viscosity, would produce motion towards the equator, not westward. The force between two floating continent masses would be an attraction. I seriously suggest that no more time should be spent on discussion of this theory until a mechanism for it is produced ; what it has done, and continues to do, is to distract attention from the serious problems of geophysics.

This is the fourth time that I have taken part in a public discussion of this theory. In each previous one a distinguised biologist or geologist has presented the case for drift, and has been followed by equally distinguished ones who have pointed out facts that it would render more difficult to explain. At present all accounts of the relevant facts are under suspicion of selection to fit a particular opinion, and I think that a time has come when a general account of the facts of past distributions of fauna and flora should be produced without regard to a particular theory. The present impasse at least suggests that some important factor has been overlooked, but might be noticed if all the data could be considered together.

The Bearing of Late-tertiary History on Vertical and Horizontal Movements of the Continents

by

Prof. S. W. Wooldridge

Recent years have made us familiar with arguments in favour of a former rapprochement of the continental masses, based upon close analogies in the details of stratigraphical succession and structural history. It is difficult to base any such arguments upon analogies in denudation-chronology. The continents separated by the Atlantic Ocean certainly show such analogies. All display ancient erosion surfaces, preserved as more or less dissected plateaux, and the successive changes of base-level thus indicated imply a denudation-chronology closely similar on the two sides of the Atlantic. Thus the work of Joseph Barrell on the ' piedmont terraces ' of the Appalachians reveals a record significantly parallel with that of Western Europe, while the ' staged plateaux ' of Africa find obvious analogues in India and throughout the Southern Hemisphere.

Since, however, many of the surfaces in

question are plausibly attributed to Tertiary erosion, and most of them, in any case, must have been fashioned under the control, direct or indirect, of marine base-levels, they have no direct relevance to the present discussion. No supporter of continental drift denies the existence of a Tertiary ancestor of the Atlantic Ocean. A marine linkage between the two sides of the supposedly widening rift would seem, indeed, to be indicated at least during the later part of Mesozoic times.

Nevertheless a consideration of the plateau morphology, particularly of the southern continents, has an indirect bearing on continental drift in the proof which it seems to give of strong vertical, epeirogenetic or plateau-building movements. We may briefly illustrate the matter by the findings of Dr. F. Dixey in Africa. In Northern Rhodesia he recognises no fewer than eight erosion surfaces. Two of them are regarded as ' fossil ' surfaces exhumed from beneath the basal beds of the Katanga and the Karroo systems respectively; with these we are not for the moment concerned. But he notes in addition a Late Jurassic peneplain (7,000–7,500 ft.), evidence of early Cretaceous planation in the major valley troughs, a late Cretaceous or early Tertiary peneplain (5,200–5,400 ft.), a Mid-Tertiary or Miocene peneplain (4,200–4,800 ft.), with subsequent late Tertiary or early Pleistocene phases of planation in the chief valleys.

There is no doubt ground for difference of opinion concerning the mutual relations of the surfaces and the dates assigned to them. But it is difficult to avoid Dixey's general conclusion that, subsequent to the folding and faulting which followed the Karroo sedimentation, there ensued successive relatively even uplifts of continental extent, each initiating a cycle of erosion culminating in general or partial planation. Renewed localised movement seems to have been deferred to a late, possibly post-Tertiary, date when warping and rifting occurred. Dixey has further indicated that the main stages of the erosional sequence appear to be legible also in South Africa.

It is difficult to conceive of successive uplifts on this scale without concurrent and complementary movements of the ocean floor akin to that ' continental foundering ' which, for so long, in accordance with prevailing fashion we have been forbidden, by some, to envisage. The nature and mechanism of the ' uplift ' may remain in doubt; its reality can hardly be gainsaid and any hypothesis which sought to treat it as absolute or unrelated to movement of the ocean floor, would, on the face of it, be untenable in terms of geophysics, and incompatible with well-attested records of geological history. It is of course clear that, in so far as such complementary foundering took place during Tertiary times, it is not directly relevant to the drift problem, but there is every indication that it may have begun much earlier and may thus afford an indication of one at least of the methods by which the continuity of Gondwanaland was broken.

Evidence drawn from geomorphology thus points suggestively if not conclusively to a type of movement compatible with the former existence and ultimate demise of land bridges. ' Up and down ' movements of the type indicated are not, in themselves, inconsistent with independent movements of drift. But they offer an alternative solution to some of the biological problems. It is important not to lose sight of the fact that biological evidences in favour of former *access* between certain of the continents are largely neutral in deciding between vertical and horizontal movements of the crust, designed, as it were, to facilitate inter-continental migration. Biologists were formerly content with the land-bridge hypothesis. Some of them have rallied to the support of continental drift without realising the ambiguity of much of the geological evidence and the grave difficulties which confront the devising of an adequate geophysical mechanism. If it becomes possible to think again in terms of land bridges and their coastal margins, the biological problems will find thereby no less ready a solution than by continental drift. It is in such a direction that the progress of our knowledge of geophysics would certainly seem to point. It is nearly forty years since Sir John Murray, quoting G. W. Littlehales, referred to ' the extreme improbability that there could be such shifting of materials in the depths of the Earth's crust as would cause the sub-

oceanic heaviness to give place to the sub-continental lightness which has been found to subsist.' But this view, widely prevalent as it became, and wholly accepted by Wegener, was based on too simple a picture of sub-crustal conditions. With two sub-crustal rock-shells only, sial and sima, ' each capable ' in Jeffreys' words ' of only one physical state,' continental foundering was difficult to conceive. The later findings of seismology, indicating three distinct rock-shells each probably capable of both vitreous and crystalline states, carry the possibility, as Jeffreys has clearly insisted, of vertical movements on a major scale. Such strong vertical movements are indeed not merely to be expected ; they are widely and unarguably evidenced by the facts of geomorphology no less than those of stratigraphical geology.

Continental drift and related topics were the subject of a discussion sponsored by Section E at the London meeting of 1931. The position as then set forth has undergone no very radical change in the years between. Then as now no demonstrably adequate mechanism for such movement was in sight, though Prof. Holmes had begun his persuasive advocacy, in qualitative terms, of the hypothesis of sub-crustal convection. As he himself has since written, ' many generations of work, geological, experimental and mathematical may well be necessary before the hypothesis can be adequately tested.' Meanwhile it is idle to overlook the evidence that great vertical movements have, in fact, taken place. Apart altogether from the evidence here reviewed such movements afford the only tenable explanation of submarine canyons for those who regard them as having been sub-aerially fashioned.

Postscript by Professor Wooldridge

Since the discussion at Birmingham, Prof. L. C. King's important paper on the World's Plainlands has been published (*Quart. Journ. Geol. Soc.*, CVI (1950), p. 101). It would not be fitting to discuss his conclusions here but it is fair to note that he writes as a supporter of continental drift and his interpretation of the great continental erosion surfaces is notably different from that implied above. It must suffice to say that while I am impressed by Prof. King's evidence and prepared to retain an open mind on the mechanism of planation, I can see no cogent argument for continental drift in the facts he adduces. Further, even if the erosion surfaces were not peneplains in the accepted sense, but ' pediments ' brought about by scarp-retreat, considerable intermittent movements of continental uplift seem none the less to be indicated.

The Relation of the Scotia Arc to Pangaea

by

Dr. J. R. F. Joyce

Introduction

In published reconstructions of Pangaea the Scotia arc[1] has constituted a serious difficulty in that its immense length fits nowhere easily into the re-assembled land-masses. Wegener (1924) in his concept of Pangaea in Palaeozoic times (*loc. cit.*, Fig. 1, p. 6) shows the arc as a short connection between the South American and Antarctican blocks, and explains its attenuation as due to the stripping-off and leaving behind, en echelon, of successive chains as the blocks advanced westwards. Du Toit (1937) in his reconstruction of Pangaea in Gondwana times shows the arc almost in its present form but folded into the gap between the South American, South African and Antarctican blocks (*loc. cit.*, Fig. 7, p. 58). King (1950), aiming at ' shortening and straightening the Palaeozoic Samfrau geosyncline,' short circuits the arc by supposing that the late Mesozoic and Tertiary fold belts pass directly from northern Graham Land to Tierra del Fuego (*loc. cit.*, Fig. 1, p. 354). Since the time of Barrow (1832) many writers have recognised the Scotia arc as the southwards extension of the Andes ; the present speculations deal with the probable role played in Pangaean geography by this important link.

The difficulties encountered by Wegener, Du Toit and King in placing the Scotia arc in their Pangaean schemes arise mainly from insufficient bathymetric data. New information, which is now available, enables its place to be suggested with greater probability even though only a minute portion of the 2,700 mile long feature is visible and this almost geologically unknown.

Physiography

The precise course of the Scotia arc (Fig. 1) has been debated, but Herdman (1932) established that this passes through the South Sandwich Islands, and his second report (1949), based on more data, shows that the feature consists of three concentric components, sometimes ill-defined, but nevertheless still recognisable except where insufficient soundings have been taken. These components are, for convenience of reference, named the inner, central and outer loops in relation to the

[1] The Scotia arc = the Southern Antilles of earlier writers (see J. M. Wordie in the Foreword to *Disc. Repts.*, XXIII, 32–162 (1945)).

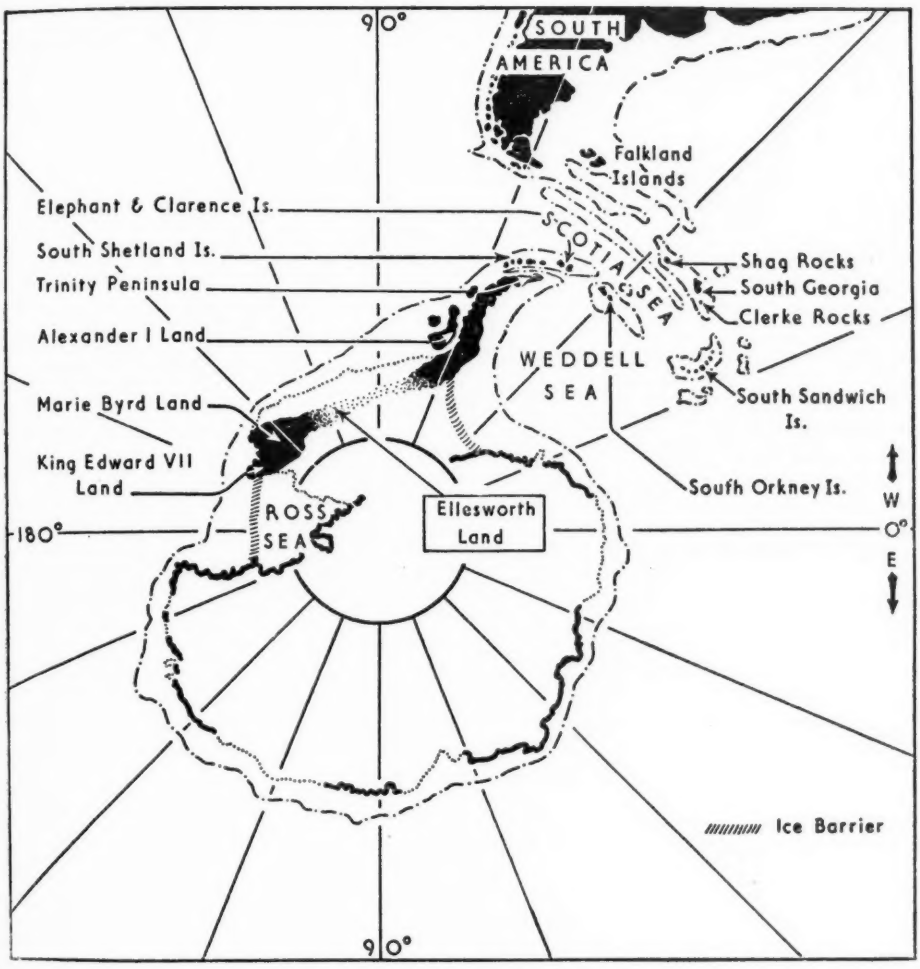

Fig. 1.—The Scotia Arc as it is to-day.

Scotia sea which is enclosed by them. This new evidence is of import in the consideration of the relation of the triple loops to the structural units of the Andes.

The *central loop* passes from Tierra del Fuego via the Burdwood Bank, Shag Rocks, South Georgia, Clerke Rock, South Sandwich Islands and South Orkney Islands to Trinity Peninsula. The chain was previously thought to continue into the South Shetland Islands, but this view is shown by Herdman's more recent evidence to be incorrect. The *outer loop* emerges from the South American continental shelf via the Falkland Islands and stretches eastwards as a continuous submarine tongue to Long. 42° W. From this point onwards it is represented by isolated sea mounts. Between the South Orkney Islands and Trinity Peninsula, however, the data from soundings are too inadequate for it to be distinguished, but somewhere east of the latter it fuses into the Graham Land platform. The *inner loop* is shown in Herdman's sections as marked irregularities on the ocean floor to the south of the central loop between Tierra del Fuego and South Georgia. West of the South Sandwich Islands the inner loop loses its identity in what is possibly a tectonically confused area, where compression may have been greatest. North of the South Orkney Islands it re-appears as a series of sea mounts, which merge to the west into a submarine ridge passing through the Elephant and Clarence Islands to the South Shetland Islands. At the western end of this group the inner loop joins the Graham Land platform.

Herdman's work provides evidence for the first time that the Scotia arc is a triple feature and not a simple linear link. This complex structure makes it a robust chain some 300 miles in width, and comparable to the Andes in

importance. The relation of the three loops to the Andean structural units can be suggested thus : the central loop is in direct continuity with the central cordillera, the outer loop, emerging from the continental shelf, is clearly related to the hinterland, and the Inner loop, while not yet established as having a physical connection, may be a continuation of the Praecordillera. From northern Graham Land the fused loops of the Scotia arc continue to King Edward VII Land to form the Antarctandes of West Antarctica.

David (1914) considered the polar plateau of East Antarctica to be a horst with a basement of metamorphic rocks of great age overlain by flat-lying Palaeozoic and Mesozoic rocks, and states that Andean folding is absent along its Pacific front. Taylor (1930) further suggested that West Antarctica is separated from this horst by a trough lying at the foot of the polar scarp and joining the Ross and Weddell seas. The nature of this trough has been debated, but recent American and British maps show that the east coast of Graham Land trends sharply westwards about Lat. 70° S. into the trough. The possibility of a passage existing between the two seas still cannot be ruled out. The extension of Graham Land through Ellesworth Land and Marie Byrd Land to King Edward VII Land forms the Pacific side of this depression.

Geology

A summary of the geological information available is set out opposite in such a form that the validity of the geographical correlation may be tested. The Scotia arc has attracted little attention from geologists in general and though perhaps cumbersome the table is apposite.

The following generalisations may be made from the data contained in the table. (1) Lower Palaeozoic orogenic movements involved the whole of the arc from Tierra del Fuego to King Edward VII Land ; in pre-Cretaceous times movements only affected it as far south as northern Graham Land, while in post-Cretaceous times evidence of mountain building beyond South Georgia is lacking. (2) Shallow water and terrestrial deposits, either sedimentary or volcanic in origin, were deposited at intervals in time and space along most of the arc from Palaeozoic times onwards. (3) Recent and active volcanoes occur as linear features in Tierra del Fuego, the South Sandwich Islands and the South Shetland Islands where the arc has its most abrupt changes in direction. These volcanoes may have some tectonic significance, and earlier sites of vulcanicity might also indicate contemporary changes in direction of the arc. (4) Fold axes and trends in the visible portions of the arc show a general parallelism with its course, which suggests that it follows the trend of old fold systems. A second structural direction, roughly NE.–SW., the significance of which is not clear, can be seen in Falkland Sound and in the South Orkney Islands. Herdman's detailed bathymetric charts also suggest that breaks in the submarine ridges follow roughly the same direction.

This evidence supports the contention that the Scotia arc with its extension into West Antarctica has persisted as a structural feature since Lower Palaeozoic times. Scanty though this is, it also lends some support to the geographical correlation between the triple loops and the Andean structural units, and places the homology of Graham Land and Tierra del Fuego in truer perspective than heretofore.

The position of the Scotia arc in Pangaea

The present reconstruction is based on the interesting agreement to be described immediately. The difference between the direct line from Cape Horn to the South Shetland Islands and the length of the arc is roughly equal to 30° of latitude, which is the distance between Antarctica and Australia. This agreement may be co-incidental, but, on the other hand, it may be significant since, if the Scotia arc is straightened in linear continuation of the Andes, Antarctica would be in juxtaposition to Australasia.

The problem of the position of the Scotia arc in Pangaea involves all the continental masses, and the reconstruction was approached in the following fashion. The edges of the present continental shelves were taken to mark the outlines of the continental blocks which formed Pangaea. Orogenic belts were extended on a factor of 3 : 1 and this was added to the forward edges of the continents to compensate for a real reduction due to orogenic compression. The present continental masses may embrace accretions through sedimentation since they drifted apart ; this was ignored as being insignificant. The width of the triple loops of the Scotia arc was also carefully considered. Some previous reconstructions show distortions of the continental blocks of a kind that would only be possible in elastic matter. The present writer believes that the sialic crust has the properties of almost rigid matter, and has therefore avoided any such distortions in his reconstruction. The continental blocks adjusted as described above were drawn on separate equal-area projection nets on a scale appropriate to the globe available. The continental outlines were then cut out of thin sheets of gelatine and transferred to the globe where they could be fitted in place.

The relationship between the *length of the Scotia* arc and the distance *between Antarctica and*

PRAECORDILLERA	CENTRAL CORDILLERA	HINTERLAND
Tierra del Fuego (*Kranck* 1938, *Thomas* 1949)		
Old Effusive series and some older rocks with Andean Diorites Highly folded	The High Metamorphic series overlain by Permo-Triassic Yaghan-Buckland series and Cretaceous Flüsch intruded by granites. Recent volcanic rocks All completely folded	Tertiary Molass overlain by younger deposits and underlain by Upper Palaeozoic and Mesozoic rocks Tertiary and younger very gently folded. Older rocks folded
INNER LOOP	CENTRAL LOOP *Burdwood Bank*	OUTER LOOP *Falkland Islands* (*Andersson* 1907, *Halle* 1912, *Baker* 1922) Archean rocks overlain by Devono-Carb. Quartzite series and Gondwana Lafonian series The Lafonian is gently folded while the older rocks are highly folded The Glossopteris flora is compared with that of South Africa
	The Shag Rocks (*Tyrrell* 1945) Never landed on, but photographs show stratified rocks (metamorphosed sediments have been dredged in the vicinity) Thought to be highly folded from the evidence of the photographs	
	South Georgia (*Ferguson* 1921, *Wordie* 1921, *Wilckens* 1937) Moderately deep water sediments of Cretaceous (Aptian) age with some intrusive and extrusive rocks to the S.E. Highly folded	
	South Sandwich Islands (*Tyrrell* 1945) Recent volcanic rocks (craters still active) reported to have Antillean rather than Andean affinities	
	South Orkney Islands (*Pirrie* 1906, *Tilley* 1935) Rocks all pre-Carb. in age. An older and younger metamorphic series with a still younger conglomeratic series Moderately highly folded	
Elephant and Clarence Islands group (*Wordie* 1921, *Holtedahl* 1929, *Tyrrell* 1945) Phyllites in the N., and rocks of higher metamorphic grade in the S. Highly folded		
South Shetland Islands (*Tyrrell* 1949) Core of older volcanic rocks and Andean Diorites flanked by younger volcanic rocks. Recent volcanic rocks on Deception Bridgman line These do not appear to be highly folded	= CENTRAL CORDILLERA *Trinity Peninsula* (*Andersson* 1906) Graywache-shale series and Jurassic volcanic rocks and lagoonal sediments Strongly folded	= HINTERLAND *Erebus and Terror gulf* (*Andersson* 1906) Cretaceous shallow water sediments, Miocene shallow water sediments and volcanic rocks Only very gently flexured
= PRAECORDILLERA *Palmer Archipelago* (*Gourdon* 1908, *Ferguson* 1921, *Barth and Holmsen* 1939) Propylitised volcanic rocks and Andean Diorites These rocks do not appear to be highly folded	*Mainland of central Graham Land* (*Ferguson* 1921, *Andersson* 1906, *Fleming* 1937, *Knowles* 1945) Descriptions of isolated exposures and rocks collected from moraines indicate metamorphic rocks. Graywacke-shales, Andean Diorites and granites No suggestion as yet as to structures	*Weddel sea coast* (*Knowles* 1945) Sediments of low metamorphic grade and Andean Diorites. Moderately folded
Alexander I Land (*Fleming* 1937, *Knowles* 1945) Jurassic shallow water sediments, but younger rocks may also be present Unfolded		
	King Edward VII Land and Marie Byrd Land (*Warner* 1945, *Passel* 1945) Metamorphosed graywache and shales and Andean Diorites Folded into broad anticlines and synclines	

Australasia mentioned above is utilised by reflexing the arc into a somewhat sinusoidal but almost linear feature. The Antarctic horst is placed against West Antarctica, thus closing the Ross-Weddell seas trough. When South America is brought into juxtaposition with Africa, Antarctica lies in the angle between the Scotia arc and the east coast of Africa. In reflexing the Scotia arc, Antarctica is thus rotated through an even greater angle than suggested by King (1950). As Australasia is brought into contact with the Australian Antarctic Territories coast, New Zealand then fits on to King Edward VII Land through Macquarie Island. With Antarctica in its new position, it is necessary to turn India to the east and not to the west as in previous reconstructions.

When Pangaea was thus assembled the position of the land mass was plotted on Lambert's Equal Area Polar projection, the pole of the projection being by arrangement near that part of Pangaea which later formed the Scotia arc. The use of a co-ordinated grid in serial reconstructions by other writers gives a false impression as to the relative stabilities of the continental blocks. It is for this reason that a grid has been omitted from Fig. 2 which shows the results of these operations.

Effects of the new arrangement of the continental masses

Space does not allow each and every agreement resulting from the new arrangement to be discussed fully, and only the most important points are outlined below. The Scotia arc in this reconstruction becomes a ' leading edge ' to that part of Pangaea which gave birth to East Antarctica ; it is also the link bonding the continental blocks of South America, Africa, Antarctica and Australasia. Further, with the true width of the Scotia arc in place, South America lies somewhat south of the position allotted to it by Wegener, and the Antilles are extended in a similar manner to that suggested for the Scotia arc. They thus form a ' leading edge ' to north-west Africa. In view of the similar roles suggested for these two ' leading edges ' Tyrrell's findings on the South Sandwich Islands volcanic rocks are of interest.

The agreement between the Scotia arc and the continental masses it bonds can never be established with thoroughness, because only an insignificant portion of its length rises above water. Some agreements are however demonstrable. The Falkland Islands have lithological and structural conformity with the Cape fold ranges of South Africa and support is also given

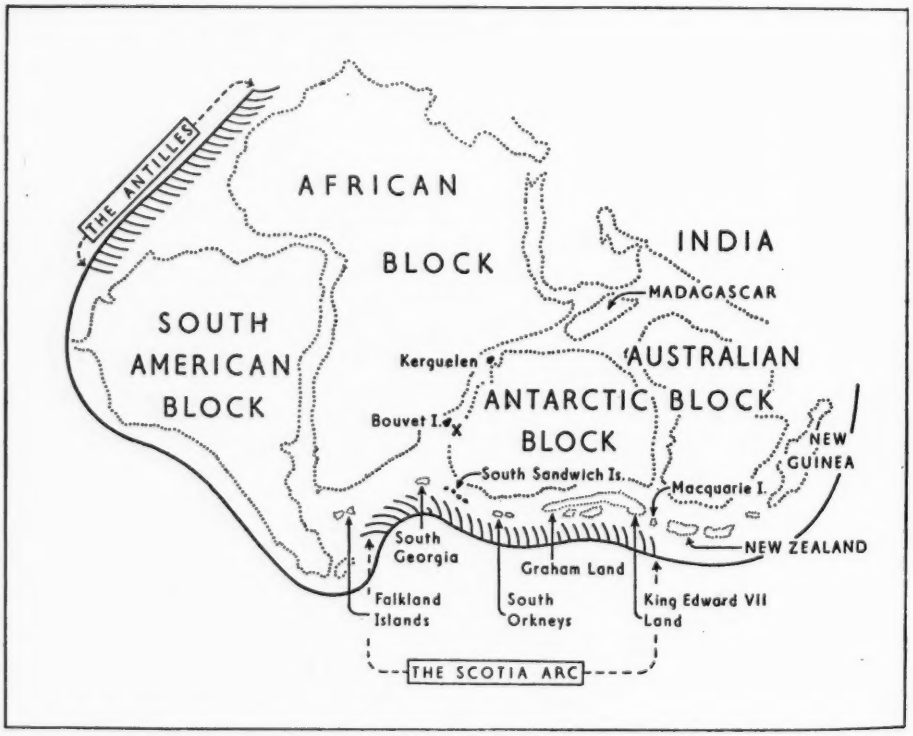

Fig. 2.—Part of Pangaea in Lower Palaeozoic times showing the Scotia Arc and part of the Antilles. The continental outlines are schematic. The pole of projection is marked by X.

by the similarity of the Glossopteris flora of the two areas (Baker 1922). The Cretaceous rocks of South Georgia are now brought into proximity to the Cretaceous beds of Portuguese East Africa, and the results of a faunal comparison, when this is available, will be of interest. The absence of marked folding in Upper Palaeozoic times in south Graham Land and West Antarctica is paralleled by a similar feature in the Antarctic horst, and Schetelig (1915) has already pointed out the close affinities of rocks from Scott's nunataks to those of the horst. In addition there appears from the general accounts available to be some agreement in the lithology of the rocks of King Edward VII Land and the southern portion of South Island, New Zealand.

The most spectacular adjustment in the reconstruction other than that proposed for the Scotia arc concerns India and may be examined in a little detail. Wegener's justification for placing India close to Africa is the grouping of areas affected by Permo-Carboniferous glaciation. The position suggested for India here has the following advantages. With India rotated slightly to the east, Kutch lies adjacent to the shallow-water carboniferous deposits of western Australia, and there is no need to postulate the improbable shore line of the upper carboniferous shallow sea as proposed by Wegener and Du Toit. At the same time the sea between the Australasian and Asian blocks figured by previous writers for Palaeozoic time is destroyed.

In two recent papers Holmes has opened the way to further study of Pre-Cambrian orogenic belts by dating those in India (Holmes [1] 1950) and Africa (Holmes [2] 1950). Although this is pioneer work, enough has been elucidated to allow the suggestion to be made that the trend lines shown for India (*loc. cit.*, Fig. 1, p. 20) and Africa (*loc. cit.*, Fig. 2, p. 90) are not favourable for an India lying close to Africa as figured by other writers. With India in the position now proposed it may be that correlation of the Pre-Cambrian trend lines must be made with Australia. This is not, however, at present possible for, although Hills (1946) has examined the broad structural outline of the western Australian Pre-Cambrian, no dating has as yet been done. When these structures are set in order, it will be interesting to see what support is given to the present reconstruction.

Conclusion

The primary claim that can be made for the proposed reconstruction is that of morphological completeness, for first there is a pleasing geometrical fit; secondly, the fold girdle to East Antarctica has been restored; thirdly, the 'sialic vacuum' of the sea thought to exist between Australia and Asia in Pangaean times has been destroyed, and lastly the difficulty of disposing of two arcuate features in the Pangaean scheme has been overcome. The suggestion is therefore made that *if Pangaea did in fact exist* then this new organisation of continental masses and island arcs *at the opening of the Palaeozoic era* is more in accord with the known data.

Acknowledgments

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SECTION J—PSYCHOLOGY

ABSTRACTS OF COMMUNICATIONS

READ TO THE SECTION AT THE ANNUAL MEETING OF THE ASSOCIATION IN BIRMINGHAM AUGUST 30 TO SEPTEMBER 6, 1950

TRAINING IN SUPERVISION AND MANAGEMENT

By Mr. A. M. B. Rule and Mr. D. H. Bramley

The effective use of production resources and man power in industry depends in the ultimate upon the quality of management. Yet, until recently, little attempt has been made in the British Isles to provide organised facilities for training supervisors and managers. Growth of interest in providing such facilities during the last decade is surveyed. Some fundamental propositions which, from the authors' experience must be taken into account in any effective scheme of management training, are discussed. These include :—

Necessity to understand the human abilities needed in supervisory and management tasks ; to establish a gradual development of experience in managing ; to integrate supervisory and management training ; to make the training programme a real test of persistence and application ; to conduct the training programme discreetly ; to accept that formal studies of supervisory and management techniques call for a new educative process of a vocational kind.

ATTITUDE TESTING

By Mr. E. G. S. Evans

The paper gave an account of recent efforts to obtain quantitative assessments of the attitudes of children to different aspects of their school work. The testing procedures and the test results were described together with some comment on their reliability and validity.

PRESIDENTIAL ADDRESS

An address on *Human affairs and the psychological point of view* was given by the President of the Section, Dr. J. C. Flugel. The full text appeared in *The Advancement of Science*, No. 26, pp. 224–237.

PSYCHOLOGICAL METHODS IN VOCATIONAL GUIDANCE—25 YEARS OF RESEARCH

By Mr. P. Smith

In 1924 Local Education Authorities were given the power to be responsible for all employment administration for young people under 18. The Birmingham Education Committee accepted these powers and devoted considerable attention to the possibilities of improving the vocational guidance given to school leavers. Research work continued for 25 years and

evidence has been produced to show that it is possible to give scientific guidance to all children leaving school provided there are trained teachers; and further that the adoption of scientific methods greatly improves the advice that can be given. As a result of the investigations appropriate Training Courses are being arranged for selected teachers and the information collected regarding the scholars is being used for educational and vocational purposes.

A vocational guidance scheme in Warrington

By Dr. Joan Wynn Reeves

In 1947–8, coinciding with the raising of the school-leaving age to 15, the Warrington Education Committee introduced vocational guidance on systematic lines for leavers from all schools in the area. The scheme has involved study of the work available, encouragement of young people's own interest in their choice, the study of their development so that some estimate of the potentialities can be made, and, finally, 'follow-up' of the young people after they have entered employment. The scheme has hinged on close co-operation between selected and trained teachers, the school medical officers and head teachers, the parents, and the employers, with the Youth Employment Officer having a nuclear function in the whole organisation of the work.

Selection for further education in the mining industry

By Mr. E. W. Hughes

It was estimated that some 20 per cent. of the 15–18 year old entrants to the Mining Industry would need to undertake further education in Technical Colleges, with a view to providing a recruiting ground for the future tradesmen and underground officials in the industry. The paper described how reports, tests and interviews were combined in pilot selection work to this end.

Over 60 field workers were trained to cover an estimated intake of 15,000 youths. Tests were designed for youths ranging from illiterate to School Certificate standard. Special attention was given to the relative merits of Information and Aptitude for Tests mechanical and electrical trades.

Presentation of technical information

A discussion on this subject was arranged by the Engineering Section. Miss M. D. Vernon spoke as a representative of the Psychology Section. Her paper appeared in *Engineering*, Sept. 8, 1950.

Effects of home background on school achievement

By Miss E. D. Fraser

An attempt was made to discover whether factors in home environment have an influence on progress in school, when other factors such as intelligence have been accounted for.

A representative sample of Aberdeen children was chosen, the home of each visited and an interview form completed for each. Intelligence test scores were known and Junior and Senior Secondary Schools provided follow-up results.

This paper gave an account of the research procedure and an interim report of results.

Intelligence, social maturity and environment

By Miss M. L. Kellmer

The concept of social maturity and a discussion of Doll's Social Maturity Scale. An outline of the research project which was conducted on intensive rather than extensive lines. The sample, consisting of 200 normal and 50 problem children, was chosen from four areas of varying cultural and economic background to ensure a representative cross section as well as to facilitate the study of the influence of these different environments. The mean intelligence and social quotients with their standard deviations are shown below.

	Working Class	Rural	Profession.	Metropol.	Child Guid.
I.Q.	98·9	100·4	111·3	106·6	96·9
Sigma	11·8	13·9	18·2	13·3	18·9
S.Q.	100·7	105·5	103·0	101·9	91·6
Sigma	10·2	6·5	9·0	10·1	11·9

On the basis of the discrepancies between I.Q. and S.Q. 40 cases were chosen

for further clinical study. The results were discussed and illustrative cases quoted.

Problem children and the concept of normality

By Prof. C. W. Valentine

Educational backwardness: Some quick results through ' remedial education.'

The need for a more precise knowledge of the ' normal ' conduct of children, before the ' problem ' child can be detected.

The confusion between ' problem ' children and ' troublesome ' children.

The vagueness of the concept of ' normality ' in the ' normal ' child at some time a ' problem ' child.

The frequency of neurotic traits among adults. Which of us is ' normal ' ?

Adults who were problem children without it being recognised.

Follow-up studies of problem children, and the need for detailed studies of individuals.

Emotional states in adolescence

By Dr. C. L. C. Burns

A study of boys and girls at the time of early adolescence reveals a certain number who are not making a success of their school career, who become difficult, dreamy, nervous or unsociable. If there is a tendency to withdrawal advice should be sought. These states seem to be prevalent among children of high intelligence.

Methods of dealing with these conditions were outlined, and examples given.

Patterns of knowledge

By Prof. G. P. Meredith

Laboratory studies of cognitive processes commonly take, for their material, elements of knowledge too small for the results to have much obvious relevance to actual problems of education and communication in general. Meanwhile human knowledge accumulates far too rapidly for rational and systematic dissemination among those who need it. This paper treated knowledge as a phenomenon in itself to be surveyed by scientific methods. A ' cartography of knowledge ' was presented in which existing patterns of knowledge could be precisely mapped. The logical and psychological properties of these patterns were discussed, with illustrations drawn from experiment. The paper concluded with indications for improved methods of knowledge-communication.

The selection of students for teacher training

By Mr. G. E. R. Burroughs

The paper dealt with an analysis of the method employed for the selection of potential teachers at the University of Birmingham since the war. The factors underlying interview performances were considered in the light of experiments conducted. An extended selection procedure incorporating written and other tests which had been used on students seeking admission in 1950 was discussed.

The scientific interests of adults

By Dr. W. E. Flood

As part of a wider study of methods for extending a popular understanding of science, the author has made investigations of (a) the relative interest in 23 branches (aspects) of science, (b) the relative appeal of 6 methods of approach. The investigations were carried out with adults voluntarily attending science lectures, an army education group, and certain Training College students. In the analysis of the results, attention was paid, *inter alia*, to possible sex-differences and age-differences. An account was given of these investigations, of the results, and of their applications.

Composite portraiture and typology

By Prof. D. Katz

This lecture was illustrated by about 70 lantern slides.

Socio-psychological factors in productivity

Prof. P. Sargant Florence

In any discussion of socio-psychological factors in productivity, psychology must, by definition, co-operate with the social sciences, and, since the issue is productivity, with economics in particular. Some points which economists would

particularly like to have elucidated to help Britain out of her critical position are :

(1) Psychological factors underlying the relative attraction of different jobs and industries, in order to control without compulsory direction the allocation of manpower particularly towards exporting trades.

(2) The relative strength of pecuniary and non-pecuniary incentives to productivity and the importance of combining the two.

(3) The interpretation of the hour-by-hour work curves (accidents, output, quantity and quality) discovered during the first world war and since re-discovered. The roles of fatigue (and hours or work), and extra-factory dusturbance.

Mr. R. Marriott

The relative strength of incentives to productivity is not fixed but varies with changing socio-psychological and material factors in the working environment. In the industrial situation pecuniary and non-pecuniary factors are almost invariably interdependent.

National and economic conditions have considerable influence, mainly indirect, but the factory worker appears to be more affected by immediate or local circumstances than by long-term or distant goals. Some recent comparative case studies demonstrate the importance of the factory ' social climate ' and suggest that, among local factors, management policies and personalities rank high.

Mr. R. G. Stansfield

The way in which a person works, and the amount he produces, is largely determined by what he is accustomed to do ; his behaviour shows an ' inertia,' and his customary performance should be considered before asking how incentives change it. The ' level of expectation ' is observed to vary, commonly over a range of the order of 2 to 1, for reasons which can only depend on ' human ' factors. The factors determining the ' level of expectation ' and its variations, the ' men ' factors affecting productivity, are susceptible of scientific study equally with the ' materials ' and ' means ' factors ; but the relevant social sciences —ranging from cultural anthropology, through economics, to psychology—are at an early stage of development relative to the physical and biological sciences. The present stage is that in which ' natural history ' observation and recording of facts is the primary need ; the stage of controlled experiment should be opening. The paper ended by comparing the order of magnitude of the present research effort in ' pure ' and ' applied ' social science, by industrial research departments and by other agencies, with the corresponding efforts in the physical and biological sciences ; and with the practical importance of the knowledge to be gained.

Dr. W. Baldamus

The paper connected past research on output fluctuations with new data on labour turnover and absenteeism, relevant to differences in type of work. Three general tendencies were discussed : average hourly output is lowest on Mondays ; absenteeism is highest on Mondays ; both turnover and absenteeism decrease with length of service. The strength of these tendencies varies significantly for different types of work. To explain this, certain non-industrial concepts (*e.g.* ' practice,' ' interest,' ' willingness,' ' boredom ') must be replaced by concepts which are specific to working conditions in an industrial society and derived from the worker's typical attitudes to work and leisure.

Diagnosis and treatment of educational backwardness

Problems and methods of dealing with retardation in Junior Schools

By Dr. W. D. Wall

1. Recent evidence of the incidence of retardation from surveys of age groups and school populations.

2. Relationship of retardation to :—(i) factors in the school ; (ii) factors in the out-of-school environment ; (iii) factors in the home.

3. Some recent experimental methods of dealing with the problem :

(i) Central Classes ;

(ii) Opportunity groups in schools ;

(iii) Special study and intensive remedial work (a) the small coaching group,

(b) individual tuition, (c) co-operation with the home.

4. The interrelationship of remedial education with social work and the work of Child Guidance Clinics.

Backwardness in the Primary School

By Miss M. M. Lindsay

Causes of variation of development in children of Primary School age, environmental, physical, emotional and intellectual.

The importance of thinking in terms of development rather than attainment.

Measure of attainment at the Primary stage.

Discussion of the particular difficulties and needs of children who are intellectually subnormal and ways of meeting their needs in the primary School.

The importance of the right group for the child as well as of methods of teaching.

The attitude of the teacher.

Investigations of the intelligibility of educational broadcasts

By Mrs. D. F. Vernon and Prof. P. E. Vernon

In an attempt to discover the main qualities that make educational broadcasts easy or difficult to understand, 50 Forces broadcasts have been assessed for 35 qualities such as concreteness-abstractness, difficulty of vocabulary, length of words and sentences, etc. Their intelligibility has been measured by scoring the numbers of main teaching points which can be reproduced by samples of adult listeners. The first paper described the methods, and the second the results of this research.

Effects of scientific broadcasts upon various educational levels in the population

By Mr. J. Trenaman

Intelligibility of three broadcasts on science as measured by immediate and remote recall of salient points and application of the ideas to new situations. Relative effects of reading and listening to the same material. Relation of interest to intelligibility. Variations in interest and intelligibility with lapse of time. Scientific material should be presented in several different ways for various educational levels in the population.

Assessing the suitability of geography textbooks

By Mr. A. E. Tubbs

The paper reviewed American research on problems of ' readability ' and related the results of such methods tried on geography textbooks with the findings of research into children's attitudes to the subject matter and teacher's evaluations of the suitability of the textbooks for school use.

SECTION H.—ANTHROPOLOGY

EXPLORATION OF CAVES IN THE DERBYSHIRE DISTRICT

INTERIM REPORT No. 22 (1950) of COMMITTEE appointed to co-operate with a Committee of the Royal Anthropological Institute in the exploration of caves in the Derbyshire district. Mr. M. C. BURKITT, *Chairman*; Mr. A. LESLIE ARMSTRONG, *Secretary*; Dr. ARTHUR COURT, Prof. H. J. FLEURE, F.R.S., Prof. D. A. E. GARROD, Mr. W. H. HANBURY, Dr. J. WILFRED JACKSON, Mr. R. U. SAYCE.

During the past year the excavation of Site No. 1, near Whitwell, N.E. Derbyshire, referred to in the 1949 Report, has been continued by Mr. Leslie Armstrong, Dr. Arthur Court and Mr. W. H. Hanbury, and is in active progress. No new work has been undertaken by the Committee, but advice has been given by the Secretary and Dr. J. W. Jackson relative to excavations in progress, or contemplated, by various people, and assistance given in the identification of specimens.

With regard to the Whitwell excavations, Mr. Leslie Armstrong reports that the season's work has been confined to *Site No. 1*, in Burnfield Grips, on the N.W. side of the valley; a small cave which has now been designated *Ash Tree Cave*. This has been excavated to a maximum depth of 4 ft. 6 in. over half the area and the Pleistocene level exposed over the remainder. At that zone the cave is an irregular triangle in plan, with an extreme length of 16 ft. and width of 9 ft. There is evidence that the width will increase considerably at a lower level.

Prior to excavation the floor of the cave consisted of tabular fragments and slabs of rock, fallen from the roof, loosely compacted together by black loamy earth. This debris was noticeably higher in the centre of the floor and fell away in a slight talus towards the rear, suggesting that a greater fall of rock had taken place in the central space. Excavation, however, revealed at a depth of 1 ft. 6 in. that the slabs covered a deposit of human bones and had been purposely piled above the bones in the form of a dome. The remains comprised most of the skeleton, including all the long bones; many vertebra; ribs; and bones of the hands and feet, but no skull or mandible. They were jumbled together and represented a typical Neolithic burial of de-fleshed bones; those of a youth of slender build, but good physique, aged 18 to 20 years. They were unaccompanied by artifacts, except two flakes of flint, which may have filtered down from above. To the right of this deposit, but separated from it and 6 in. lower, was another one, comprising a clavicle; a few phalanges and vertebra; and a mandible of exceptionally robust and 'primitive' type. The general character of these remains proclaimed them not part of the group first discovered. The mandible is an interesting archaeological specimen because of the primitive characters it exhibits; particularly in the width of the ascending ramus, the extreme shallowness of the sigmoid notch, and general robustness. It is also abnormal in the dentition, having possessed only three, instead of four, incisor teeth. Professor R. W. Lovel, of King's College, Newcastle, has examined the mandible and is of the opinion that the missing incisor is a central one and may have been lost earlier in life, although there are only three sockets now present. Sir Arthur Keith, who has also examined it, says that 'congenital absence of the lower central incisor is a very rare occurrence . . . but, to me, congenital absence is the more likely explanation.' Sir Arthur Keith also states that the features

of the mandible indicate a young and strong man of about 24 years of age. The evidence of stratification denotes a Neolithic date for the specimen. On the same horizon scattered human remains have occurred throughout the cave and these include the mandible of an infant.

In the upper portion of the rocky debris sherds of Roman and Iron Age pottery were found, numerous ' pot-boilers,' charcoal and a few flakes of flint. In places, sherds of pottery had infiltrated to a depth of one foot between the stones. Artifacts of flint and stone, pounders, animal bones, charcoal and other evidence of a sparse occupation of the cave were abundant down to the level of the underlying brown loam. Some of this material was probably contemporary with the traces of Mesolithic occupation noted in the section of the rock-shelter outside the cave, but no artifacts definitely assignable to that period have, so far, been discovered.

The brown sandy loam is glacial drift which, as observed in the trial cut made in 1949, had flowed into the interior of the cave, and the extent of its penetration has now been established. The cave is entered by a short entrance passage, approximately at right angles with the long axis of the cave ; at the entrance to the interior the thickness of the drift was 2 ft. 6 in., decreasing to 1 ft. 6 in. at the wall opposite. Down the length of the cave it rapidly thinned out, and 10 ft. from the entrance became indistinguishable in appearance from the red cave earth over which it had entered.

This drift contained fragments of abraded limestone, Bunter, and other erratic pebbles, and formed a hard compact layer. It was archaeologically sterile except at one point, in the entrance passage, where a hole had been scooped into it to receive a deposit of incinerated human remains.

The red cave earth underlying the glacial drift is of Pleistocene date, but proved to be only a thin stratum 4 to 6 in. in thickness. The portion excavated has yielded flint artifacts of Creswellian type ; fragments of split bone, some of which have been utilised ; two bone tools ; a stone pounder ; ' pot-boilers ' and charcoal. Both artifacts and animal remains are few in number, and the occupation appears to have been a casual one. The fauna includes horse ; bison ; reindeer and pig. This zone rests upon a bed of yellow cave earth, very compact, stony and calcareous, the depth of which has not yet been ascertained. It exhibits traces of severe water-logging. The upper stratum of this deposit has yielded several fragments of highly mineralised split animal bones ; charcoal ; quartzite pebbles ; and a quartzite artifact of Mousterian type, similar to examples from the Mousterian levels of the Pin Hole Cave, Creswell.

The Committee desires to be reappointed.

M. C. Burkitt, *Chairman*
A. L. Armstrong, *Secretary.*

July 31, 1950.

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